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TRANSACTIONS

OF THE

AMERICAN INSTITUTE

OF

CHEMICAL ENGINEERS

VOLUME IX

1916

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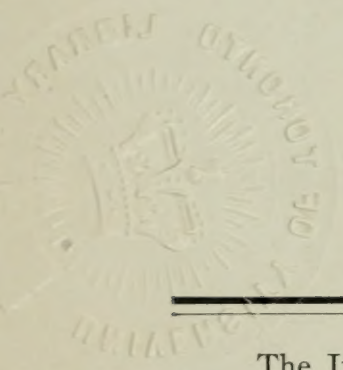

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AMERICAN INSTITUTE OF
CHEMICAL ENGINEERS

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TRANSACTIONS OF THE AMERICAN INSTITUTE OF CHEMICAL ENGINEERS

UNPREPAREDNESS

By PRESIDENT GEO. D. ROSENGARTEN

Read at the New York Meeting, January 10, 1917

When by unforeseen circumstances a nation is suddenly plunged into conditions depriving it of materials necessary for its welfare, it becomes an eminent duty of such a people to take immediate steps to attempt to restore the supply of the wanting essentials. Since history ever repeats itself, the assumption can at once be made that such conditions must have been taken under advisement long before there was any evidence of their becoming actualities, but the presumable optimistic trait in human nature and without which we would not be able to live through a happy existence, will procrastinate the evil day, with the intention of relying on the strength of the country when the necessity arises.

Recent events, however, have proved that this is no easy matter and the effort made in this direction, by making an inventory of our supplies, brings us to face the question of crude material, a question which is of the utmost importance in order that our stocks may be sustained. The United States is a country of immense resources, but owing to territorial advantages or seasonal conditions, it follows that some of the basic needs are entirely wanting or so limited that they may be considered a negligible quantity. No matter if we are surrounded by a high protective duty or laboring under a tariff for revenue simply, at least that in name, which obviously means nothing at all, instances will always remain where desirable commodities are lacking. Among the various expediences suggested, the consideration of the question of crude material has been,

to a large extent, neglected, and a very serious condition confronts us, if the very material from which the finished goods must be manufactured is placed beyond our reach. The eternal slogan "preparedness" is the real foundation for the prevention of such conditions, or at least to reduce them to a minimum. It is so easy to preach; but this is not sufficient, it is the practice we need. There are not only the much-mooted coal tar dyes, but other articles which have not had the publicity but are just as deserving. The coal tar dyes of course derived all their fame from the fact that we were dependent for them on Germany, and when the source of supply was shut off the prominence afforded them was amazing. Presuming that sulphur had not been obtainable in this country during the European crisis, can we for a moment believe that we would have enjoyed anything like the opportunity offered for prosperity,—a forced prosperity in truth and due to no cause of internal workings, but entirely and absolutely to European conditions. The very contemplation of the thought of what might have been, had we been dependent for sulphur on foreign sources, is sufficient to bring horror to any chemical manufacturer. Fortunately we were not compelled to combat with this situation, but the grave consequences are prominently brought to mind as to our situation if we were to be deprived from enjoying foreign commercial intercourse. The basic reflection is the problem how crude material is to be obtained to sustain our industries and the secondary consideration is the inventoring of our supplies.

It is quite evident that the New Tariff Commission must be deeply involved in the consideration of the question of crude material and that the industries must assist in this respect, since they are much more cognizant of their necessities. Under the condition existing, while hostilities in Europe endure, it would seem doubtful if any commission can really approximate the need of protection for American industries. The present exaggerated situations do not offer a stable basis for even a shrewd surmise towards consequent events, and certainly are far from lending themselves as a solid foundation for the establishment of what really may be necessary for the future and lasting prosperity of this country. The prominent need is that the new ventures which have been brought about by the war must receive sufficient nursing and that the industries already established must enjoy protection in the same measure to insure our independence and security from foreign inroads.

THE HUMAN SIDE OF THE DEVELOPMENT OF CHEMICAL INDUSTRY

By PRESIDENT G. W. THOMPSON

Read at the New York Meeting, January 13, 1917

NUMEROUS writers and speakers have called frequent attention to the great service which chemistry has rendered to humanity. In every field of industry she has been active, every product of man's labor has felt her magic touch. Modern civilization can be measured most accurately by considering the influence which chemistry has had upon industry and industrial operations. It is unnecessary here to tell what she has done. We are more concerned with the future than with the past, and judging the future by the past, the benefits which chemistry during the coming years will confer upon humanity will be infinitely valuable. On us, her votaries, is imposed the task of laying out the course of her progress, preparing and making the way easy for her and heralding her triumphant march. It is the purpose of this paper to indicate how this may best be done.

It should be perfectly obvious that the purpose of all industry is to satisfy the desires of human beings. This is as true of chemical industry as it is of any other industry. The chemist renders human service. He is a human being himself and his service to human beings makes him a part of a great democratic commonwealth in which every member is a factor in the development of the whole. The chemist is only a fraction in this commonwealth but the more he is organically a part of it the greater is his power of service. Like everyone else he does not exist by himself alone but is dependent for his existence upon every other human being that is a part of the social organism.

In the last analysis all of the relations between human beings are mental relations. The instruments of connection between human beings may be material but the relation itself is spiritual. In so far as the chemist renders service he does so by impressing himself on

other people and affecting them either directly through personal contact or indirectly through the products of labor in the fabrication of which he has had a part. Human relationship, therefore, is the end toward which we are perforce all compelled to work. Each man must impress other people and he must in turn be impressed by them. There is no such thing as independence. We are all dependent, and in seeking for what we may nominally call independence we are simply shifting our dependence from one point to another. It is foolish to talk about being independent and it is unwise to seek independence, because it can not be obtained. Still more important than this: it is best for us that we should not seek independence but that each one of us should seek to be dependent upon every one else, thereby conforming to the best conception of human relationships and to the economic law according to which the best service only can be rendered.

What we propose, therefore, is that the chemist should strive against anything that would tend to isolate him and separate him from his fellowmen. What we will try to show is the means by which he can avoid these tendencies; how he may more effectively integrate himself into the social organization.

Perhaps what we have in mind can best be visualized by considering here what has been believed by some to have interfered with the growth of chemistry in this and other countries. For instance, the cry has come from England that technically educated men have not been sought for as the occupants of high official and business positions. It has been claimed that a scientific education did not help towards social or public advancement, that in England a man with a classical education was sought after, not the scientific man. Assuming this to be so, it is perfectly clear that the advancement of the chemist in England has been retarded by a lack of public appreciation of the service which he renders or can render to the nation. What, then, is the chemist in England going to do about it? Apparently he is dissatisfied, which in itself is all right, but being dissatisfied does not alone change inexorable facts. In every chemical problem that confronts the chemist when facts stand in his way he does not get excited about them but seeks to surmount them by careful study and the devising of ways and means. This should be the attitude of the chemist in England, and by organized publicity—advertising if you will—he should seek to change public opinion so that it will accord to him a higher recognition.

That the English chemist is taking this view is indicated by a recent address by G. G. Henderson before the Chemical Section of the British Association for the Advancement of Science, Newcastle-on-Tyne, 1916. In discussing the synthetic production of nitric acid from the air and the great advantage it would be to England, he laments that England has done nothing in the matter except to appoint a Committee to consider possibilities. Then he goes on to say, "This case is only too typical of many others. A number of different causes have contributed to bring about this state of affairs, and the responsibility for it is assigned by some to the government, by others to the chemical manufacturers, and by still others to the professors of chemistry. I think, however, it will be generally admitted that the root of the matter is to be found in the general ignorance of and indifference to the methods and results of scientific work which characterize the people of this country." He adds, however, that he thinks that the English leaders in science have done all that lay in their power to awaken the country to the inevitable and deplorable results of this form of "sleeping sickness" and that he believes that a brighter day is dawning and that if they rise to the occasion now, chemistry in that country will attain the position of importance which is its due.

Let us take another case. Germany has made great advances in chemistry. Some think that this is due to her system of education and probably this is partly true. Some think that it is due to the farsighted wisdom of public men. This, too, is probably partly true, but the real success of chemistry in Germany in my own opinion has been due to its greater popular appreciation. Historians have long since dropped the idea that kings and rulers in general amount to very much in the progress of a nation, and have adopted the broader conception that progress ultimately is in the people of a nation, their developing thoughts, their appreciation of the world that is about them.

The lesson to us in the United States is this—that if we wish chemistry to become a more potent factor in the industrial growth of our country we must take such steps as are necessary to bring about a popular appreciation of its value. Universities will help, government aid will help, but these too depend upon popular appreciation both in their beginning and in their execution; after all they are mere incidents to fundamental active causes. Create a demand for universities and universities will rise, as it were, over night; create

a demand for governmental interest and governmental interest will come quickly and effectively, but demand is born of popular appreciation.

Mr. Elwood Hendrick in an article that appeared not very long ago in one of our popular journals emphasized this point in urging the popularization of chemistry, and he expressed the thought that chemists would not come into what they believed to be their own until this was done. Mr. Hendrick's suggestion would indicate the advantage of chemists generally advertising their profession, not necessarily, however, in the form of paid advertisements, but in properly written articles for the popular press. The desirability of such a procedure properly carried out is fairly obvious, subject to some limitations, which we will consider further on.

As an illustration of what we can accomplish in this direction I would refer to Robert Kennedy Duncan's writings (particularly his *"Chemistry of Commerce"*), as having been exceedingly efficient in popularizing chemical knowledge, and to the fact that Duncan's efforts in this direction probably resulted in the establishment of the Mellon Institute. As another example of a very commendable effort to popularize chemistry, I would refer to the Chemical Expositions, the second of which was held last September in the City of New York. When the first Exposition was proposed, many chemists expressed a great deal of doubt as to its wisdom and practicability, but as the time approached for the opening of the Exposition, its value to chemists became more clearly recognized and it received from them a very generous and hearty support. So successful was the first Exposition that immediately a second one was projected. This received even more generous support and two of our largest chemical societies decided to hold meetings at the same time. The public press has been full of matter descriptive of the Exposition itself and the accomplishments of chemistry thereby illustrated, and gave much space to the papers and discussions, presented at the meetings of the two chemical societies. These Expositions were distinctly advertising in their character and great expenditures with the expectation of profitable return were made by the exhibitors themselves.

Our National Departments of Agriculture and Commerce have done much in popularizing chemistry. The work Dr. Wiley started in the Department of Agriculture has extended the knowledge of chemistry in popular fields through the publication of numerous bul-

letins and leaflets. More recently the Department of Commerce has, through special investigators, been instrumental in increasing the knowledge of the relation of chemistry to industry. Similar work has been done to some extent by the Department of the Interior. All of these efforts have been of great advantage to chemists.

It seems hardly necessary that I should go further in emphasizing positive aspects of this phase of my subject. Let us consider now some of the criticisms that have been offered on work of this kind. The most general criticism is that when an attempt is made to popularize a science such as chemistry many unscientific statements will be made and true representations will often be incorrectly interpreted by the public. This is undoubtedly so. Erroneous ideas have been promulgated in this way, but the chance to err is not confined to popular presentations. We know that many errors have crept into scientific literature necessitating laborious effort later on for their correction. To err is human, and the danger that error may occur should not act as a preventive of public statements, whether these statements are made in the scientific or popular press.

Another objection comes from those who hold that the profession of chemistry is of a very exalted character and that the serious chemist will not descend to ordinary popularizing effort. Our own Code of Ethics would prevent the chemist from advertising in a sensational manner, and requires the professional chemist to publish scientific papers in the technical journals before they are given to the popular press. It seems to me that our Code of Ethics is correct in this respect. We do not want to advocate sensationalism and we do want to build up our technical journals, but after the requirements of our Code have been complied with there still seems to be a field open to the chemist in which he can with perfect propriety popularize his science.

The advocates of the study of pure chemistry are also apt to look down upon this popularizing scheme. To them popular science is offensive and it may be that some of them can hardly be expected to sympathize with the views I am here presenting. The student of pure science is, however, almost in a class by himself. The utmost that he does in affecting the world's processes is by furnishing information, studies and generalizations to be applied by the practical chemist in furthering the interests of the entire world. Far be it from me to depreciate the value of the pure scientist. We all draw upon him for help in our practical efforts. It

must be admitted, however, that he lies essentially outside of the plan that we are proposing. He indulges more or less in an esoteric culture and his light, though it is not hidden, is reflected to the world at large only by those who can practically direct that light to the illumination of ordinary things. In spite, therefore, of all that may be said against the popularization of chemistry, I am still convinced of the wisdom of such a purpose, because it is only by so doing that even the student of pure chemistry will be given an opportunity to carry on his studies. So firm is my belief in this that I would extend this effort so that it would reach and include the most humble members of society.

In order to present in picture form, as it were, the dependence of the chemist for his success upon the popular appreciation of chemistry, let me present the following sketch. A chemist makes what he believes is a discovery of great financial value. If he has money enough of his own he may be able to make that discovery a source of financial profit, but in so doing he will have to be something in addition to a chemist, for the largest part of his effort will be along lines which are essentially non-chemical, involving the organization of business, the advertising of the product or process, and its sale for use. In proportion as the public is capable of appreciating what he has accomplished, to just that extent will he make a financial success. Suppose, on the other hand, this chemist is not a man of means and wants to interest financial people in his discovery. As things are to-day he will first encounter skepticism, even ridicule, and if finally he is successful in having his discovery appreciated by financial interests, it will be because he has been able to convince them of its human aspects, and it may be that before he is through a great part of the value which he has seen in his discovery will have been absorbed by what the financial interests think is their proper return. The ordinary business man of to-day very naturally thinks that his business ability is more valuable than the technical ability of the chemist. He wants the lion's share. There is no use in closing our eyes to this fact. Many a poor inventor or discoverer has been so discouraged in his early efforts for recognition that he has been almost ready to drop his entire proposition. He says the blame lies on an unappreciating public, and this is true, but if it is true, how incumbent it is upon the chemist to make the public appreciative. He can do this only by studying the public, finding wherein the public should be educated, and recognizing all

its limitations, work with wisdom toward the accomplishment of his purpose.

I think that I have said enough along this particular line to indicate to you my views as to what the chemist should do in the direction of trying to develop a popular appreciation of his work. If there is truth in what I say I am quite sure the open mind of the average chemist will discover it. If I am wrong he will soon find the vulnerable point in my argument. I would like now to pass to another aspect of this question, which relates more particularly to the chemist himself. What I have said so far deals for the most part with the attitude of mind of the chemist toward the public. What I propose to say now deals with the attitude of mind of the chemist towards himself. I have referred to a relation existing between the chemist and the public. In this relation there are two parties, the public on the one hand, and the chemist on the other. The problem before us is: What should the chemist do to develop and improve his attitude of mind towards himself? This is an exceedingly difficult subject, and if it were not that I am fortified by a belief in its importance, I would not dare to speak upon it. I cannot think of anyone holding a higher opinion of chemistry as a profession than I do, and all that I am saying is actuated by a profound belief that the chemist is only beginning now to climb the ladder that reaches to a power of service unequaled by any other profession.

The delicacy of this subject is such that I feel impelled to approach it in the form of questions. In putting these questions, let us consider first some facts, and then we can come to the questions themselves. Let us first consider the young chemist just leaving his college or university. Like all students graduating in various courses, he has yet to learn what business life really is. They think they know what it is, but we know that there is no college or university that teaches it. The difference between the chemist and most other graduates is noticeable in this: The chemist enters a more or less isolated laboratory; most of the others go directly into the commercial world. The chemist in this laboratory—be it analytical or research—is not dealing with people but with inanimate material things. He may stay in this laboratory a few months, a few years, or for his lifetime, as the case may be. He may be doing invaluable work, but—here is the question: Is he taking his proper place as a man in the world of men? Is he not too much

isolated? Is he in his occupation gaining directly in his power over other men? Is his social instinct being developed or is it being retarded in its growth?

If we think that laboratory chemists as a general class are taking their proper place in the world in which mind reacts upon mind, there is nothing more to be said; but if we think with his intelligence and education, the chemist has a mission outside of the indirect service he professionally renders to society, then our question leads us to urge that the barriers which ordinarily isolate the laboratory chemist should be broken down so that he can get out more into the pulsating business world. He may not be able to do this alone, and perhaps the duty devolves upon us; perhaps there are some of us who have it in our power to help bring about this liberation. If we want chemists to have a greater personal influence in the world, and by this we mean to include those chemists who are subordinate to us individually, is it not proper for us to ask ourselves whether at times we do not rather selfishly stand in their way? Cannot those of us who are employers of chemists give our employees more opportunity to become familiar with the commercial aspects of the work they do? If we do this, will they not become better and more profitable employees? In our dealings with non-technical men representing large business interests, could we not more regularly and persistently ask to be brought into contact with their chemists if they have them, thereby helping to pull them out of their seclusion? If we are asked to organize a laboratory in a commercial concern, could we not say that the work of the chemist would be greatly helped if it were so arranged that he would know the actual use to which his reports were to be put? Could we not do something in urging that the laboratories in which such chemists are expected to work should not be hidden away in inaccessible places? Could we not do many things suggested by the occasion to break the bonds which the young chemist, led by false hopes, may have often slowly forged? Is it not possible that with our colleges to-day filled with students of chemistry, these students should be advised against accepting positions of an isolated character?

You will see that I am basing my argument on two hypotheses, which are intimately related. One is that social relationship is one of the great purposes of life, and the second is that without such social relationship no individual can really be successful in the development of any important enterprise. I have argued, therefore,

that the chemist's success depends upon a social recognition of his worth, and second, that the chemist should enter into as broad social relationship as possible. In popular terms one of these means that it pays to advertise, and the other means that the chemist should get out upon the broad highway.

I propose now to say a few words on the disadvantage which is sure to result from antagonisms. I propose to advocate in all our dealings a conciliatory spirit. It would seem almost unnecessary to say that if the chemist is to advertise, he should avoid offending the persons who read his advertisements, just as it is perfectly obvious that the salesman who is trying to sell goods should not make himself *persona non grata* to the purchaser. A good salesman—and that is what we should all try to be in one form or another—subordinates his own views and makes them as inconspicuous as possible, but no less effective. What he tries to do is to find out the views of his customer so that he can furnish the customer with what he desires. I wonder if sometimes chemists are not a little more pugnacious than they should be. I sympathize with the feeling of an analytical chemist who will set his results up against the world and insist that he is right, but many years of experience have taught me that the chemist who does this without regard for the effect upon other chemists is doing a very foolish thing. I believe it is a good rule never to put anybody "in wrong." I believe it is far better, even in the case of commercial opponents to help the commercial opponent out of his difficulties rather than to push him in deeper. Again, are we not at times a little too virulent in asserting our ideas respecting public officials—administrative, legislative and judicial. They are human beings just as we are, they are representatives of the whole people, and if their views differ somewhat from ours, is it not very much better to work patiently and steadily to change their views, directing them into constructive paths, rather than to confirm them in their views and drive them to unwise extremes by opposing them vigorously?

Let us take as an illustration a subject on which perhaps chemists are divided at the present time, although the division may be by no means equal. Many chemists are seeking to have our National Government help the chemical industry to become more firmly established in this country. Some think it can be done by subsidies, others by protective tariffs, others by embargoes, and so on. Now I would not argue in this meeting for or against any of these propo-

sitions. I simply say that if you are in favor of any one of them it is unwise because he opposes you to incur the antagonism of any public official, even if he be of an opposite political faith. Practically all public officials have their ears to the ground; they are trying to find out what their constituents want, and no one should blame them for this. In so far as they are representatives they are only doing their duty. It is perfectly proper for us to try to convert a public official to our way of thinking. It is still more proper for us to try to convert the people he represents to our way of thinking, but I consider it the height of foolishness to attribute to public officials dishonest purposes, ignorance, stubbornness and all the other sins of commission and omission. My point is very clear—such accusations do not lead you anywhere but into trouble.

I believe firmly that the obstacles which we meet in our social acts in endeavoring to get our plans consummated do not differ essentially from the obstacles which the chemist meets in the ordinary routine of his work. If he finds a chemical reaction is not progressing satisfactorily, he does not close his eyes to this fact and denounce some devil or anthropomorphic god because it has interfered with his work. He tries to find the cause of the trouble and remove it. So I say that if you have social obstacles to overcome, find their cause and remove it, bearing in mind that there is no easy way by which this can be accomplished—in fact that there is only one way—education backed by goodfellowship.

We have heard in recent years a great deal said about efficiency; it may be that some foolishness has been uttered. The word "efficiency" is used in various ways. We have the efficiency of a process considered by itself; we have this efficiency as compared with other processes; we have mechanical, chemical and human efficiency. I am interested primarily in this paper in human efficiency. What does it mean? Broadly speaking it means co-operation without friction. Perhaps we should say that co-operation cannot be had with friction. Of course it would be absurd for me to attempt to solve the eternal problem of human friction in a paper of this kind. All I can do is to indicate some ways by which it can be reduced. While the realization of frictionless humanity is far away, symptoms of it have certainly appeared at times. Efforts in the direction of reducing human friction conspicuously numerous at this time are to be found illustrated in factory welfare work, Workman's Compensation Acts, pension systems, etc. All of these have

been justified by the increase in human efficiency they have produced. A man whose welfare is of no account is not a good workman.

Human efficiency, however, demands right men in the right places. The difficulty that the employer experiences is in knowing whether he is picking the right man for the place or not. If the man has been educated rightly the employer is better able to form an opinion. Particularly is this true regarding foremen and the higher grades of labor. Graduates of such institutions as give technical education are made more nearly right for places in chemical and other industries. More general industrial education such as that being advanced in some states will undoubtedly add to human efficiency. Even then the making of a man suitable for a given position comes after he commences employment and is largely the work of the employer himself.

Industries do not merely require men, however, they are complicated machines of which the men form a part. The proper fitting and adjusting of these machines to the men employed is the big problem only partly worked out as yet. Each industry has its own requirements, these requirements continually changing with the growth of society. Who has not been face to face with the requirements of an industry? These requirements are almost vitalized by competition. Industries come and industries go, they are planted and they are reaped, but their volume increases from year to year. We must not, however, allow ourselves to think of industries and industrial processes as though they were other than inanimate things. We should not waste our tears over their rejection when they are worn out, provided others take their place. This is growth—the sloughing off of the old and the putting on of the new.

The chemical engineer has a great deal to do with the development and administration of new processes. Many an old fogy has damned the chemical engineer, who has rendered his processes and his equipment antiquated and idle. The mechanical engineer also has received his malediction. We are sorry for the old fogy and would, if it were possible, provide insurance for him, but we would do nothing to retard the fate of his equipment.

The development of a new process is the result of many separate factors. New processes are like the waves of the sea; they cannot rise much higher than the level of the sea itself. They rest on general industrial growth and are not much above the level of that growth. This is true of all processes, chemical processes included.

Their growth depends upon the growth of the mechanical art, its methods of fabrication, and all the materials that enter into construction. It depends upon highly developed foundry practice, mining, building construction, farming and transportation, and last, but not least, all those branches of human development which are manifested in the home, social organization, governments and international relations. I emphasize this human factor because so many processes have had to wait years for the time to ripen for their introduction, indicating, in other words, that not alone should our endeavors be directed to the development of processes, but they should also be directed in a general effort to accelerate the coming of the fullness of time. The fullness of time means that the social level has been sufficiently raised.

New processes are of two kinds: first, those which provide improved methods for making old and well-known products, and second, those which involve the making of new products to take the place of old, or new products which serve entirely new purposes. A new process which is intended to provide methods for making an old product, if it is to be successful, must involve economy of some sort, economy of material, investment, time or labor. Necessity creates the demand for such new processes. Competition, domestic and foreign, is the impelling force. The wise manufacturer recognizes that the development of a new process may be his salvation. It is impossible in this paper to discuss all the demands which compel the invention of a new process. We will discuss only the human or labor element.

If labor here in the United States is higher in price per unit of product than it is abroad, it is manifest that with free competition the manufacturer must so modify his process that this item in cost will not militate against him. He may not be willing to modify his process, and his first instinct may be to limit the competition by a protective tariff or some similar restrictive measure. One objection to this, except as a last resort, is that it removes some of the pressure which makes for improvement of process. We may dislike the pressure but we know after all that it is a good thing. We cannot allow anything to interfere with such pressure as makes improvement necessary. Such improvement may be possible. It is much wiser to insist that it is possible than to insist that it is not. Success is often accomplished by doing what is apparently impossible, but assuming that the manufacturer cannot get the protection he wants,

how will he go about improving his process? We will offer only a few suggestions.

The first consists in manufacturing in large units. By this means overhead charges are reduced and materials can be handled more cheaply by mechanical means, thereby either directly or indirectly reducing the cost of the labor factor. In this direction it would appear that the United States has been pre-eminently successful; there is probably no country in the world which equals us in this line of development. The second consists in subdivision and specialization of labor. It is obvious to all that it is through this method that most of our industries in this country have been successful. The third is radical development of process. If we take a given process and consider all of the operations, step by step, we are able tentatively to classify these steps into two kinds—the essential and the apparently necessary. The apparently necessary step is not by any means an essential step. An apparently necessary step is one that the state of the art has not eliminated. An essential step is one that we feel justified in saying cannot be eliminated, no matter what the state of the art is. By a radical development of process it may be possible to eliminate apparently necessary steps. The burning down of a house in order to roast a pig was in the mind of the Chinaman of fiction an apparently necessary step. It was, however, eliminated by the Efficiency Engineer of his day. Fourth, by doing by machinery what was theretofore done by labor. An essential step in a process may still appear essential, although instead of performing the step by manual labor it is done by machinery. This much does not involve the elimination of a step. Doing by machinery what was theretofore done by labor is the obvious method of reducing labor costs. It usually entails, however, additional cost for investment, power, and repairs, and also the employment of a higher class of labor. It is perfectly obvious that where labor is cheap machinery is unnecessary, but that where labor is high, competition compels the use of machinery, and that this is the proper and natural order of things.

This is perhaps to you a somewhat academic presentation of the methods to be followed in studying how a process can be improved. If I should go into the methods by which new processes for making old or new products might be worked out, it is probable that this study would be still more academic, and I will not attempt here to touch upon that branch of the subject. What you may consider

academic in what I have said, however, has a very practical bearing upon the general subject I am endeavoring to present. As I have already indicated, I am interested in human efficiency. Mechanical efficiency that does not lead to human efficiency is of rare occurrence. Considered in the most general economic terms it is doubtful if it ever occurs. It is natural for us to think of human efficiency as a means rather than an end, but I would reverse this relation and ask you to consider human efficiency as more of an end than a means. It is desirable that we should have high-priced labor because high-priced labor usually means greater intelligence and higher character.

A more highly developed social organism is one of the noblest objects for which one can work, and all efforts spent on the development of its parts is well worth while. If you believe with me that an appreciative public is necessary for the proper growth of the chemical industry, you will also agree that we need a public which shall have the power of appreciation which can only come as a result of the general raising of the level of social conditions.

The chemist by working for improvement in processes involving a saving of labor, and at the same time involving the employment of a better grade of labor is contributing his part to the better development of a high social organism. If the chemist, however, simply endeavors to improve a process without regard to the possibility of that improved process involving the employment of a higher type of labor, then his work is incomplete. It is true that his process may with cheap labor be able to manufacture products which are used by the entire community, and which consequently can be sold at a reduced figure and thereby benefit society, but many difficulties will lie in his path if he does not at the time that he reduces the quantity of labor necessary in the process, also create a demand for a higher quality. I therefore ask those of you who are engaged in the development of processes, such development being forced by competition and the requirements of the industry, to consider whether it would not be better for you to put your mind in the attitude that in your development of process you may possibly be able to get better results by the use of more intelligent and higher priced labor than you could by sticking to low-priced labor and possibly handicapping yourself and your process and preventing its fullest development.

I have endeavored in this paper to present several aspects of a very large and important subject. Some parts of it may appeal to

you and other parts may excite your opposition. In the main, however, I believe all of these parts are related to each other consistently and logically. If you believe as I do, that a better social organism is the highest purpose for which anyone can work, you will accept what I have said and include in the proposition many extensions on which I have not touched. If, however, your minds are more practical, I think you will accept many of the things I have presented to you purely on account of their practical value. Therefore from whatever standpoint you approach the subject, am I not correct in stating that practical business success in all lines of business, including those involving the use of chemistry, is dependent upon popular appreciation, is dependent upon social growth, on which the power of appreciation rests, and that everything which tends to increase the power of appreciation helps appreciation itself, and that anything that increases appreciation increases the chance of success of a business enterprise. Or to put it negatively, anything that interferes with an increasing power of appreciation, anything that tends to prevent appreciation or creates those antagonisms which operate against appreciation is pretty sure to hinder the growth of a business enterprise.

If this fundamental proposition is accepted, we can readily see how important it is for the chemist in attempting to build up chemical industry in the broad sense to do everything in his power to raise the general level of human knowledge and education and of social conditions, for it is that level that determines the power of appreciation. Assuming this power of appreciation to exist and to be growing in strength, you will also, I think, agree with me that the chemist should do everything in his power to create a greater appreciation of the value of chemistry in solving the problems of industrial life, for it is only through such appreciation that the chemist himself will be able to exert fully that ability for service which we all believe he possesses. But if the chemist is to impress himself on the public through legitimate advertising, should he not also endeavor to develop that attitude of mind which makes him continually a factor in social growth? Should he not break away from all influences that tend to isolate him, and that, if they do not make him a peculiar being, at least have the effect of preventing him from having the fullest influences over others? Should we not do everything in our power to broaden the horizon of the chemist, to increase, as it were, his circulating power in the social organism? Should we not,

as chemists, by very simple methods do all we can to pull chemists individually out of all such surroundings which tend to narrow and handicap them?

Now having done all of these things, should we not go a step further and recognize the peculiar position which the chemical engineer occupies in the development of chemical processes? Our minds naturally operate discontinuously. We think of a chemical process and its development as an independent thing, as though it were not a part of all of the other processes continually operating in society. We think that with the development of a process our work is ended; we think that with cheaper manufacture of given products we can sit down with satisfaction and consider our efforts successful. But philosophers tell us that there is no such thing as discontinuity, that everything runs into everything else continuously and is a part of a complete whole. Some fancifully have said that if a man could destroy a single atom, the universe would fall to pieces. This is fanciful but it expresses a true conception. There is such a thing as solidarity—not the solidarity of labor, not the solidarity of industry, but more surely the solidarity of society. All things operate in circles. We have continuous production and continuous consumption, in which material things circulate around and around, never completely stopping. This is so of all human affairs. Effects are unending. All the ends we work for are but the means to other ends. The development of a process was the end yesterday, but to-day it becomes a means—a means by which the social level is raised, which in its turn becomes a means by which other processes are developed.

This is why I have expressed my opinion that in the development of a process it can hardly ever be said to be complete unless it results in the substitution of quality of labor for quantity, whenever such a development has for its purpose the saving of labor. Development of a better quality of labor is essential if we are seeking broadly to increase the power of appreciation on the part of the public. It would be manifestly foolish for me to advocate that chemists should seek to impress Hottentots and Fiji Islanders with the power of service in chemistry. It would take centuries of education to bring them up to the point where we of America are to-day. It is equally foolish to think that we can expect as much appreciation from common labor as we can from intelligent labor. In saying this I make the assumption with which I hope you will agree—that

every member of society, the ignorant and the wise, the poor and the rich, the bad and the good, the sick and the well, are all integrally a part of society and each one has his individual influence in determining the average social level which measures the power of appreciation.

I have endeavored in this paper to avoid taking any position which is negative in its character. I do not believe that there is such a thing as a negative force; the nearest we come to it is inertia. I have endeavored to make positive suggestions which I submit for your consideration with the daring hope that these suggestions may find lodging places, temporarily it may be, in your minds, and if, after these ideas have stayed in your minds for a short time, they then take their flight, I have the still more daring hope that after you have forgotten them, they may, though unconsciously to you, have left some faint imprint.

DISCUSSION

Dr. A. C. LANGMUIR, acting as Chairman, called the meeting to order and said:

We had on the program for Wednesday evening a paper by Dr. G. W. Thompson on "The Human Side of the Development of Chemical Industries," and on account of the length of the program on that evening Dr. Thompson kindly consented to postpone his paper until this evening. I am sure we will all be very much interested in hearing this paper, which certainly treats chemical engineering and chemical industries from an entirely new viewpoint. I call upon President Thompson with the greatest interest to present this paper.

Dr. Thompson read his paper on "The Human Side of the Development of Chemical Industries."

The CHAIRMAN: I think we all agree that Dr. Thompson's paper is a most unusual one, and represents on his part a great deal of careful thought, and I trust that the paper will be published widely and have an extensive circulation. In such case, it ought to have a very stimulating effect, I think, upon chemical engineers and chemists. It was only four years ago, some of you may remember, that a Borough President here in New York City, in welcoming the members of the Eighth International Congress of Applied Chemistry, referred to the gentlemen present as "These here druggists." That mistake could scarcely take place to-day. I think the province

of chemistry is such, and the public recognition of chemistry is so much improved, that no politician to-day would be guilty of such a mistake.

A story appeared in the papers a few weeks ago that has some bearing on one of the points raised in Dr. Thompson's paper. A gentleman called at a doctor's house one night at half past one, a windy and rainy night and rang the bell. The doctor came down stairs and said, "What do you want?" The man replied, "I have a sick friend who lives out in the suburbs. What will you charge to go out and see him?" The doctor said, "My charge for night calls is $3." "That is all right, doctor," the man replied. "I would like to have you go out. Will you take me along?" "Certainly," said the doctor, "step into the car." So, they drove out through the wind and rain, and wet and mud, and when they reached their destination the man handed the doctor $3 and said, "Here's your fee, I am very much obliged." "But where is the sick man?" asked the doctor. "There is no sick man," replied the man. "I couldn't get a taxicab to take me out here for less than $5 or $6, and you were willing to do it for $3."

We chemists are also apt to hold ourselves too cheaply, and it is a point that perhaps was not brought out in Dr. Thompson's paper that if we will only hold our heads up, and stand up for our rights, public appreciation will follow.

Dr. Frerichs, you have had considerable experience as a manufacturer and employer. Perhaps you have some comments to make on Dr. Thompson's paper.

Dr. F. W. FRERICHS: I have been a manufacturer for about thirty-five years, and have always had in mind to improve the position of laboring men. When an employee of the Mallinckrodt Chemical Works, I instituted a mutual aid association governed by the employees themselves. There was a small amount of money deducted every week from the wages—I believe it was one cent from a dollar—and the amount of money collected in this way was doubled by the firm, so that in fact there was at the disposal of this fund about two per cent. of the entire wages paid out to employees. This money was used for relief in case of accident, and in ordinary sickness. We employed a physician who visited in case of sickness the employees, and their families, at a very low rate. While a physician ordinarily charged $1 for a visit, he charged us 50 cents. We kept that up for many years, and relieved the laboring men considerably. At the

same time we saved up money in our treasury. This fund was under my supervision. I was superintendent of the works, and the fund was distributed by a committee elected by the employees, I being Chairman of this Committee. Later on I went into my own business, and have insured all my employees collectively under a workmen's collective insurance, have secured aid for them in case of accident, and even in case of sickness. A great majority of the laboring men have highly appreciated this course and I have never had a strike. I have now workmen in my employ who have been with me as long as twenty-five years, and about one-third of my men have been with me from ten to fifteen years I believe that if you improve the position of your employees you improve their efficiency and the quality of their work.

The CHAIRMAN: Mr. Hooker, perhaps you have something to say.

A. H. HOOKER: One point particularly interested me in Dr. Thompson's speech. I refer to the bettering of the quality of labor in connection with the development of a process. It seems to me that as we improve our methods of manufacture by introducing labor-saving machinery, that we increase, of necessity, the investment per unit of employees. This throws on these men a greater responsibility, and good judgment will point out the desirability of selecting a higher type of man for the work. The rule should therefore be that as we improve our manufacturing process, and particularly so when introducing labor-saving machinery, improve also the quality of labor, paying them better wages, allowing them to live in better environments, under better living conditions at home. These very conditions will improve and raise the standard even of the same man.

Dr. F. W. FRERICHS: For a long time, I have been operating my works twenty-four hours a day, in two shifts, having each of the men work twelve hours. It appeared to me that the apparatus was not so well attended to as I wished, and I changed about four years ago from a two shift to a three shift day, having every man work only eight hours. I left the same men on the job, and gave them in the first year only slightly less for eight hours' work than they had formerly earned for twelve hours. As a result, I had three men who were able to work one apparatus instead of two, and in case of sickness or accident, I always had two men who, for a time, could work for twelve hours, and when the third man returned to work, the men would work eight hours again. I found this to be

a great advantage. The men themselves are more efficient, they pay more attention to their work, and the efficiency of the machinery, and apparatus, has been greater since I introduced the eight-hour day.

The CHAIRMAN: You have probably heard the rhyme:

"Eight hours' work,
Eight hours' play,
Eight hours' rest,
Eight dollars a day."

When we come to that ideal, perhaps we can have the millennium, and perhaps we can have none but university graduates running our plants.

REPORT OF COMMITTEE ON CHEMICAL ENGINEERING EDUCATION

Read at the San Francisco Meeting, August 25, 1915

Prof. JAMES R. WITHROW: Mr. Chairman and members of the Institute: I will not make any formal report this morning, but just make a few statements for the information of the members who probably recollect that we decided at the Troy meeting to pursue no active work further with the committee until after we had heard what the Carnegie Foundation was going to do and find out what they did do when they got under way. You probably also recollect from a former report of your committee that the Carnegie Foundation is working in co-operation with a joint committee of Engineering Societies arranged by the Society for the Promotion of Engineering Education, and that our Institute is not represented on this committee. Upon hearing a statement of this fact at the Troy meeting, Prof. Green, Head of the Mechanical Engineering Department, Rensselaer Polytechnic Institute, privately volunteered to take this up with the Society for the Promotion of Engineering Education with the view of having this Institute represented on their joint committee which was working in co-operation with the Carnegie Foundation for the Advancement of Teaching. Prof. Green did this at the Eastern meeting of the S. P. E. E., Princeton, June 1914, but on account of the delicacy of the situation, the American Chemical Society being the one which was represented on that committee to co-operate in the matter of chemical engineering education, Prof. Green could not push the matter very actively, but he secured a resolution, which was passed, turning the matter over to the committee of the Society for the Promotion of Engineering Education for its own decision. What the committee did I do not know. We have never heard from them in the matter.

During the course of the year, however, the Carnegie Foundation became active and Prof. Mann, who was selected to visit a number of institutions in this country to go into the matter of

engineering education, came to the Ohio State University in the course of his trip, and in conversation with him, for I had the opportunity of spending a portion of three days with him, I brought up the matter and he took it up through the Foundation as soon as he returned to New York. The consequence of Dr. Mann's action was that President Rosengarten was requested by the Carnegie Foundation to appoint the Chairman of your committee a representative of our Institute to confer with the Carnegie Foundation at any time they would call for advice. In other words, the Institute now has been made officially a part of this consulting committee. Since that was done I have heard nothing further. I do not believe the Foundation has called the committee into consultation since that date. This probably is natural, because the committee very likely was in active co-operation with them before Prof. Mann started out. Prof. Mann, as far as I know, is still actively engaged, or at least was until June, in his work and what the results will be I do not know.

I might state as a personal impression, that Prof. Mann's attitude of mind was very satisfactory. I think it would be very satisfactory to the members of the Institute also, judging from our previous work and opinions expressed on this subject. I noticed that, perhaps wisely, he did not care to go actively into the details of the technical side of engineering at all.

It seemed impossible for me to interest him, for instance, in following the details of my own technical work. This was not his purpose. He expressed great interest in the technical work, but did not want to spend any time, then at least, on its details. He was actively interested, however, in what the conditions were for the students in the way of a foundation for technical work which I think was a good sign; at least, he wanted to know how chemistry was being taught as a foundation for chemical engineering and all other kinds of engineering, how the languages were being handled, how mathematics were being handled, and the same with physics, he himself being a Professor of Physics in the University of Chicago.

REPORT OF THE COMMITTEE ON CHEMICAL ENGINEERING EDUCATION

Read at the Baltimore Meeting, Jan. 12, 1916

THE AMERICAN INSTITUTE OF CHEMICAL ENGINEERS:

GENTLEMEN: Since the report made by your Chairman at the San Francisco meeting of the Institute, the Carnegie Foundation for the Advancement of Teaching has called twice upon the representative of our Institute on the Carnegie Foundation's Advisory Board for Engineering Education, for discussion of and suggestion upon the progress and future of the Foundation's investigation of the Teaching of Engineering.

The first communication from the Foundation referred to the questionnaire involving some nine questions, which was sent out by the Foundation in the early part of the summer of 1915. This was in co-operation with the Joint Committee on Engineering Education of the various Engineering Societies, our Institute being represented on this advisory committee. The Foundation's communication summarized the results or answers obtained to this questionnaire and requested comment, criticism, and suggestion as to how this data could be used most effectively in the report of the Foundation. It was the opinion of your representative upon the Joint Committee and was so expressed to the Carnegie Foundation that this last summer was not a good time for adequate replies from the chemical engineers. The questionnaire was sent out really much later to our Institute than to the others. Discussing the answers obtained from all sources it was evident that the questionnaire could only be considered as merely a preliminary one of general character. It will be evident to any one reading the summary of replies compiled by the Foundation and appended to this report that the essentials to engineering success, for instance, emphasized as important by one engineer may never have been thought of by a different engineer replying to the same question. Therefore, this does not signify that any one engineer laid no stress or did not

believe in the importance of the omitted ideas. This being evident it was suggested by your representative that the topics mentioned by the different engineers replying to the questionnaire be presented, as such, to the membership of the different engineering societies for vote on the relative merits of these topics. In this way only can we receive an idea of the relative importance of the different suggestions in the minds of the professional engineer and avoid the error caused by assuming the omission of a topic to be a vote against that topic, as always results in weighing the vote on a questionnaire where the voter suggests the topic himself. For instance, 15 engineers voted that graduates possessed adequate ability to apply their mathematics and 39 stated they did not. The rest of the 1400 voters do not mention this subject, therefore we have no idea of their opinion in the matter. Similar instances might be given with regard to qualities necessary to success, and other portions of the questionnaire. Your representative, therefore, suggested a second communication to the members of the different engineering societies for vote on specific items and particularly on the relative importance of the items in the two columns of the abstract of the Foundation's Summary, which is also appended to this report, and relates to approval or criticism of the schools and the factors which make for success in engineering.

It should be stated to the Institute at this point that although the number of replies to the Carnegie questionnaire is small (6.41%), yet apparently those from whom they come are men who have been successful engineers, have positions of authority, are experienced in employing engineering graduates and have pronounced opinions on some of the questions raised. It is interesting to note therefore how similar the ideas expressed by these engineers are to the ideas emphasized so many times in our own Institute when discussing this subject of education. For instance, the vote upon the Carnegie questionnaire showed that the schools are strongest in imparting technical or scientific knowledge and imparting the scientific attitude and that they are weakest in imparting business sense, ability to apply theory to practice and power of expression. It is interesting also to note that of the factors making for success in engineering, these engineers place greatest emphasis upon character, self-reliance and independence and understanding of men.

To the above communication by your representative no direct

reply was received from the Foundation, but shortly afterwards the foundation sent its second communication, which indicated that the general opinion of the Advisory Committee was that the circular letter previously sent out to the members of the National Engineering Societies brought rather meager returns and was of doubtful value as far as representing the views of the profession as a whole is concerned and suggesting that the abstract of the summary of replies to the questionnaire be sent to the local Engineering Clubs throughout the country asking them to report on it after spending an evening discussing it, and enclosing a copy of the proposed circular which might be sent to the Engineering Clubs for this discussion. This communication and suggested circular is appended to this report. It calls attention to the 16 groups of fundamental factors which are suggested as determining the success of young graduates in engineering and asks a vote upon their relative importance.

To this communication your representative replied that the idea was good and would have at least the advantage of stirring up, throughout the country, a discussion of Engineering Education among the professional men themselves, thus stimulating interest in the matter, and the net result in the long run would be helpful to Engineering Education as a whole. It was suggested also that some of the topics mentioned in the reply of your representative to the former communication of the Foundation as suitable for a basis of a future questionnaire, might be included in this communication to the local Engineering Clubs.

Because these communications from the Foundation were to the Advisory Committee of the Foundation and did not involve any policy or opinion of our Institute no meeting of the committee on Education has been held (until the conferences with the various members this morning) since that in Philadelphia a year ago. The policy of awaiting the outcome of the Carnegie investigation placed upon your chairman the duty of keeping in touch with the Foundation. The communications of the Foundation are therefore hereby laid before the Institute as a matter of record.

We desire in closing to call attention to the poor showing made by the Institute in the Foundation's list of replies.

We recommend that the Institute, if it so desires, authorize that its representative on the Carnegie Foundation's Advisory Board be requested to procure if possible copies of the Founda-

tion's new circular for distribution to local engineering clubs and that the Secretary of our Institute be requested to send it out and earnestly request a prompt vote of our membership upon the same.

Respectfully submitted,

JAMES R. WITHROW, Chr.;
SAM'L L. SADTLER,
A. D. LITTLE.

APPENDIX TO REPORT

I. QUESTIONNAIRE SENT OUT BY THE CARNEGIE FOUNDATION FOR THE ADVANCEMENT OF TEACHING.

To the Members of the Engineering Societies, represented on the Joint Committee:

GENTLEMEN: The Joint Committee on Engineering Education of the National Engineering Societies, as given above, and the Carnegie Foundation for the Advancement of Teaching have together undertaken to make a comprehensive study of Engineering Education. A wide and representative expression of opinions and ideas from the members of the profession who are in active practice is fundamental to the proper execution of this task. The interests of the engineering profession will be materially served by you if you will send answers to the following questions.

Yours very truly,

C. R. MANN.

1. What are the particular points at which the work of the engineering schools is effective and successful?
2. What are the particular points at which the work of the engineering schools is least effective and successful?
3. What specific experiences illustrate the conclusions expressed in your answers to questions 1 and 2?
4. What are the most important factors in determining probable success or failure in engineering?
5. When examining a candidate for a prospective job, what sort of information is best calculated to assist in judging his fitness and probable success? Give samples.
6. What sort of work, what specific jobs, what kind of experiences are most effective in testing a man for fitness and probable success in engineering? Give samples.

7. Have you ever found a knowledge of either French, German or Spanish essential to your professional career?
8. Should a reading knowledge of one of the modern languages be required of every engineer? Why?
9. Make specific suggestions for the improvement and strengthening of engineering schools.

Returned by .....

Address .....

September 29, 1915.

My dear Prof. Withrow:

Enclosed we are sending you a summary of the replies received from engineers to our circular letter sent out last spring. We shall be glad to receive from you comments and criticisms together with suggestions as to how this data may be used most effectively in the report.

Yours very truly,
C. R. Mann.

Professor J. R. Withrow,
Ohio State University,
Columbus, Ohio.

Returns from Circular Letter Sent to Engineers

	No. sent	No. returned	Percentage
C. E.	7,707	789	10.23
E. E.	7,747	374	4.82
M. E.	6,142	199	3.25
Ch. E.	219	7	3.19
Unclassified		31	
Total	21,815	1,400	6.41

Further information concerning the sources of the answers may be obtained from a study of the replies to Question 3, namely:

Question 3. What specific experiences illustrate conclusions expressed in your answers to questions 1 and 2?

No attempt was made to answer this question in 239 of the replies received. Of the remaining 1161, 1117 contain statements of personal experiences which are too general and varied to permit

of summarization. These statements are valuable because they show that although the number of replies is relatively small (6.41%), yet those from whom they come are men who have been successful in engineering, who have attained positions of authority in which they have had wide experience in employing college graduates, and who therefore have well defined ideas on the questions raised. Question 3 is thus placed first in order to bring out this fact.

In addition to these general statements of personal experiences the following facts were mentioned as a basis for the opinions expressed in 44 of the answers. The numbers in parenthesis indicate the number of times that the statement occurs.

Engineers as a rule do not frame the policy for public improvements, but are hired as help (17); engineers seldom serve in the legislature or on public commissions (1). In engineering meetings ten per cent of the members do the talking (2); engineering literature shows how engineers fail to convey their meaning (5); engineers cannot convince an audience (1). Only about half of the engineering graduates follow their specialty (8); there are not enough positions open to graduates of engineering colleges (2). Men trained in pure science make as successful engineers as those trained in engineering (2); Taylor's "Art of Cutting Metals" is a glorification of the laboratory method (1). Many mentally brilliant engineers have no greater earning power than carpenter's foremen (2); engineers take business courses in correspondence schools (1); few engineers arrive under the age of forty (1). Can an engineer combat a lawsuit? (1).

Question 1. What are the particular points at which the work of the engineering schools is effective and successful?

Question 2. What are the particular points at which the work of the engineering schools is least effective and successful?

Question 9. Make specific suggestions for the improvement and strengthening of engineering schools.

These three questions are summarized together because it was found that the suggestions made in reply to No. 9 were really statements of opinion concerning the effectiveness of the schools. The *first* column contains the points that were mentioned for approval or criticism; the *second* gives the number who consider the schools effective and successful at these points; the *third* column gives the number who consider the school ineffective at these

points. The numbers in the *fourth* column are obtained by subtracting those in column 3 from those in column 2 and thus indicate the net vote on each point. The numbers in the *last* column indicate what percentage of the 1400 voters this net vote is, and thus indicate roughly the weight of opinion for or against each point.

	More effective	Less effective	More—Less	Percentage
TECHNICAL KNOWLEDGE, drill in theory, theoretical training, grounding in technical subjects, teaching theoretical phases or elements of engineering, theoretical book knowledge (444-72), training in mathematics and mathematical analysis (254-15), teaching fundamental principles, knowledge of fundamental facts, basis engineering principles, laying of solid foundation in pure science (343-223), knowledge of physics (81-1), knowledge of mechanics (70-4), familiarity with the sources of engineering information, bibliography, ability to obtain information (89-12)	1281	327	954	68.1
SCIENTIFIC THINKING (36-23), analysis (73-18), method of attack (42-10), of acquiring knowledge (3-6), of solving theoretical problems (46), of research work (41-24), of observation (35), openmindedness (6-5), logical thinking (59), reasoning power (70-36), mental discipline (40-1), general mental training (25-1), habits of study (-8), how to learn (39)	483	138	345	24.6

	More effective	Less effective	More—Less	Percentage
AMBITION (9), enthusiasm for engineering (17), desire for engineering knowledge (3), pride in his work (1), alertness (3)	33	...	33	2.4
PRESTIGE (2), cheap technical labor for corporations (1), method of earning a living (2), cramming for examinations (1), making undemocratic materialists (1), advertising school (1), college yells (1)	9	...	9	0.6
FAITH IN LAW	2	...	2	0.1
SELF-RELIANCE (31-67), decision, leadership (2), sense of authority (1), perseverance (25-8), responsibility (3)	62	75	—13	—1.0
INDUSTRY (7-6), willingness to work (17), discipline (1-15), hard knocks (1-2), dirty work (1-21)	27	44	—17	—1.2
CHARACTER (8-38), personality, integrity (9)	17	38	—21	—1.5
EFFICIENCY (1-57), good use of time (1-20), get results (11), economy (2), short methods (42), concentration (25), executive ability (6-37), scientific management (-43), system and order (33), practical method (11)	132	157	925	—1.8
PHYSIQUE, physical training hygiene	1	33	—32	—2.3

	More effective	Less effective	More—Less	Percentage
ENGINEERING SPIRIT AND IDEALS (5-7), ideals of life (6-1), high purpose, professional ideals (7–15), understanding of ultimate aim of engineering (4-10), motivated work (2-48).	24	81	—57	—4.1
LEGAL AND POLITICAL TRAINING (-66), public life (-46), civics (3-2)	3	114	—111	—7.9
INDEPENDENCE (13-87), initiative (8-49), resourcefulness (10-26), fairness (2), imagination (3-13), promptness (5-3), determination (1), decision (2), loyalty (1)	45	178	—133	—9.5
HUMILITY (-3), more to learn (1-79), begin at bottom (1), subordination (7-24), morality (-36)	9	142	—133	—9.5
ACCURACY, precision (40-92), detail (7), thoroughness (11-110), neatness (4)	62	202	—140	—10.0
ENGINEERING JUDGMENT (-45), perspective (1-57), common sense (1-33), relative degree of accuracy required, proportion (-18)	2	153	—151	—10.8
INDIVIDUAL INSTRUCTION (-47), personal contact, vocational guidance (-83), weeding out unfit (5-59), overcrowded classes (-20)	5	209	—204	—14.6
ABILITY TO UNDERSTAND MEN (58-7), to handle men (2-188), to judge character (-15), to mix well (2-5), tact, social address (3-57)	65	272	—207	—14.8

	More effective	Less effective	More—Less	Percentage
BREADTH OF VIEW (16-49), general culture (10-7), general education (-254), relations of engineering to other human activities (58), general perspective of life (-56), humanities (5-10)....	89	376	—287	—20.5
TEACHERS TRAINED in practical work and in constant touch with it, training teachers of engineering (3-299), inbreeding in faculty (-14)	3	313	—310	—22.1
TOO MANY SUBJECTS, too much descriptive work (-86), too much useless information (-34), too much detail (-22), too much busy work (-2), too much specialization (10-143), narrowness of technical training (3-29), systematic arrangement of curriculum (1), subjects chosen to please professors rather than students (-10), co-operation among departments (-22)	14	348	—334	—23.8
POWER OF EXPRESSION, use of English, ability to express himself in speech and writing, public speaking (18-455)	18	455	—437	—31.3
APPLICATION OF THEORY TO PRACTICE (59-563), ability to apply mathematics (15-39) and knowledge, solution of indeterminate problems, field methods (26), use of instruments (17-3), practical experimental work (9), technique of engineering (40), use of tools (4), artisans' details, industrial methods (-33), knowledge of				

	More effective	Less effective	More—Less	Percentage
materials (13-13) and shop methods (24-28), methods of construction (19-19), mechanical drawing (38-36)	264	734	—470	—33.6
BUSINESS SENSE (3), commercial address, bookkeeping (-467), business principles, economics of engineering, business-like methods, lack of office and works atmosphere (-25)	3	492	—489	—35.0

Question 4. What are the most important factors in determining probable success or failure in engineering?

Question 5. When examining a candidate for a prospective job, what sort of information is best calculated to assist in judging his fitness and probable success? Give samples.

Question 6. What sort of work, what specific jobs, what kind of experiences are most effective in testing a man for fitness and probable success in engineering? Give samples.

These three questions are summarized together so far as the factors that make for success in engineering are concerned because these factors were mentioned in the answers to all three. The following table contains these factors grouped about leading ideas, together with the number who mentioned them and the percentage of the whole.

	Number	Percentage
CHARACTER (208), integrity and honesty (308), personality (312)	828	58.8
SELF-RELIANCE (94), DECISION (10), responsibility (152), perseverance, persistence (169), grit, gumption (12), nerve (11), force (15), determination (44), enterprise (4), sand (1), firmness (8), endurance (10), ability to endure hardships (87), courage (27), loyalty (32), fidelity (9), reliability (49)	734	52.4

	Number	Percentage
ABILITY TO UNDERSTAND MEN (273), to handle men (158), tact (94), unity and brotherhood (5), ability to co-operate (44), to judge men, to gain and keep the confidence of men (33), adaptability (68)	675	48.2
ENGINEERING SPIRIT (4), ideals of engineering (12), natural bent, innate ability (268), capacity (6), aptitude (20), mechanical ability (22), temperament (46), imagination (40), foresight (21), aim—self or work (177)	616	44.0
INDEPENDENCE (43), initiative (180), inventiveness (67), ingenuity, originality (53), creative ability (6), aggressiveness, push (48), resourcefulness (144), "pep" (2), versatility (15)	558	41.1
JUDGMENT (238), discernment (9), common sense (190), balance (30), perspective, proportion (87)	554	39.5
INDUSTRY (108), energy (95), application (60), concentration (44), willingness to work, capacity for work, how to do dirty work (195)	502	35.8
TECHNICAL KNOWLEDGE, grasp of fundamentals, knowledge of theory (440), technical training, knowledge of subject (38)	478	34.1
SCIENTIFIC ATTITUDE, logical thinking (151), reasoning power, analysis (104), ability to face facts (3), to observe (42), to profit by failure (11), optimism (4), openmindedness (45), disinterestedness (2), studiousness (47)	409	29.2
ACCURACY (156), precision, care for detail (78), thoroughness (83), neatness (11)	328	23.4
EFFICIENCY (58), executive ability (115), promptness, speed (58), system and order (17), methods of work (7)	255	18.2
BUSINESS SENSE, commercial training, knowledge of business (219), value of money (12), how to market his services (10)	241	17.2

	Number	Percentage
BREADTH OF VIEW, general education (228) ...	229	16.2
AMBITION (72), enthusiasm (34), interest (90), alertness (21), ...	217	15.5
POWER OF EXPRESSION, ability to convince, to converse well	217	15.5
HUMILITY (19), willingness to start at the bottom (27), patience (43), conscientiousness (15), sincerity (8), obedience (4), morality (34), religion (3), self control (15), lack of big head (8).....	176	12.6
APPLICATION OF THEORY TO PRACTICE (157)	157	11.2
PHYSIQUE, physical health (152).....	152	10.8
LUCK IN STRIKING RIGHT JOB (79), opportunity (14), influential family and friends (18)...	111	7.9
SAME AS IN OTHER WORK (43)	43	3.0
MEMORY (17)	17	1.2

The following table contains a summary of the replies to Question 5 as to the sort of information best calculated to assist in judging fitness and probable success in engineering. Since 219 failed to answer this question, the percentages are made on the basis of 1181 votes.

	Number	Percentage
Experience, record of experience, success in previous work, rate of promotion	695	58.8
Personal impression, general appearance, address, quick wit, ability in presenting his case, conversational ability	436	36.8
Previous training, schooling, college record, preparation	414	35.0
Personal interests, personal habits, associates, boyhood experiences, spend spare time, outside reading	210	17.7
Opinions of others on character, on work, references	195	16.5
Aim—self or work, money or use, common welfare, attitude towards service	177	14.9

	Number	Percentage
Length of service, reasons for leaving jobs	59	4.0
Family history	37	3.1
Ability to pass examination on requirements	36	3.1
Opinion of self and his own work	23	1.8
Did he work his way through school? Finances	11	0.8
Understanding by employer of what sort of characteristics are needed	3	0.2

Question 6 is not answered in 381 of the letters. Of the 1019 replies, 860 suggest as the thing most effective in testing a man's fitness and probable success any work requiring one or more of the factors of success summarized under Question 4. In addition 149 make specific suggestions as follows. The number in parentheses indicate the frequency of occurrence.

Emergency work (39); trouble work, correct others' failures, solve problems not clearly stated, correct incomplete drawings (36); write a report that carries conviction (32); analysis of unfamiliar problem (13); special task in his specialty (9); apprentice work (8); running tests on machines (7); make inventory or bill of material (3); search for data or needed information (1); speak before an audience (1).

Question 7. Have you ever found a knowledge of either French, German or Spanish essential to your professional career?

Of the 1363 engineers who answered this question, 1000 or 72.6% confess that they have never found a knowledge of any foreign language essential to their professional career; 153 have found foreign languages essential in a general way, 57 more specify German, 35 French, 73 Spanish, 43 both French and German, and 2 Italian. The engineers thus show more unanimity on this point, which is one of mere fact, than on any other.

Question 8. Should a reading knowledge of one of the modern languages be required of every engineer? Why?

Notwithstanding the marked consensus of opinion expressed in the answers to Question 7, 678 or 49.8% still think that at least one foreign language should be required in the engineering schools. Of these 183, or 27% state as their reason for this the conventional idea that such *study develops culture and breadth of view.* Six hundred and eighty-five, or 50.2%, think that foreign languages should not be required in the engineering schools chiefly because

the returns to the student are not at all commensurate with the time spent. Of these 685, 84 are willing to have foreign language included as an elective, and three specify that it should be required for entrance.

ABSTRACT OF THE SUMMARY

It appears from the detailed summary of the replies that both the points mentioned for approval or criticism of the schools and the factors that make for success in engineering fall naturally into eighteen groups, each descriptive of an important element in the education of the engineer. In column 1 of the following table, these eighteen fundamental factors are arranged in the order of the engineers' opinions of the present efficiency of the schools in these particulars. In column 2 these same factors are arranged in the order of the engineers' opinions of their importance as factors of success in engineering. Some idea of the engineers' judgment of the relative importance of these factors may be obtained from the numbers which give the net vote and the percentages of the whole.

	Net vote	Percentage
1. Technical knowledge	954	68.1
2. Scientific attitude	345	24.6
3. Ambition, enthusiasm	33	2.4
4. Self-reliance	—13	—1.0
5. Industry	—17	—1.2
6. Character	—21	—1.5
7. Efficiency	—25	—1.8
8. Physique	—32	—2.3
9. Engineering spirit	—57	—4.1
10. Independence	—133	—9.5
11. Humility, patience	—133	—9.5
12. Accuracy	—140	—10.0
13. Judgment	—151	—10.8
14. Understanding of men	—207	—14.8
15. Breadth of view	—287	—20.5
16. Power of expression	—437	—31.3
17. Application of theory to practice	—470	—33.6
18. Business sense	—489	—35.0

	Total vote	Percentage
1. Character	828	58.8
2. Self-reliance	734	52.4
3. Understanding of men	675	48.2
4. Engineering spirit	616	44.0
5. Independence	558	39.9
6. Judgment	554	39.5
7. Industry	502	35.8
8. Technical knowledge	478	34.1
9. Scientific attitude	409	29.2
10. Accuracy	328	23.4
11. Efficiency	255	18.2
12. Business sense	241	17.2
13. Breadth of view	228	16.2
14. Ambition, enthusiasm	217	15.5
15. Power of expression	217	15.5
16. Humility, patience	176	12.6
17. Application of theory to practice	157	11.2
18. Physique	152	10.8

December 13, 1915.

To the Members of the Joint Committee:

It is generally felt that the circular letter sent out last spring to the members of the national engineering societies has brought a rather meager return, since only about 6.5% of the membership responded. This raises the question whether the summary, a copy of which was sent to you last October, really represents the view of the profession as a whole. For this reason, the Boston members of the committee have suggested that it might be useful to distribute the abstract of the summary of these letters to the local engineering clubs and ask them the return a report on it after spending an evening discussing it. This would have the advantage of letting the profession know that the work is being pushed, of interesting them more directly in the work, and of setting them to thinking along the lines of the report. I enclose a copy of the proposed circular which it is suggested might be sent to the local clubs for the purpose specified. I shall be glad to hear whether you approve of this plan or not, and if so, what changes you would like to have

made in the form or the matter of the proposed circular. A prompt reply would materially expedite the work of the committee.

Very truly yours,

C. R. MANN.

SAMPLE OF LETTER TO BE SENT TO ENGINEERING CLUBS

To the Members of the Local Engineering Clubs:

Last spring a circular letter was sent to the members of the National Engineering Societies for the purpose of giving practicing engineers an opportunity to define for the schools the aims and ideals of engineering education. Since replies were received from only 1500 engineers, the Joint Committee on Engineering Education is doubtful whether the summary of these letters really represents accurately the judgment of the engineering profession.

The qualities which these 1500 engineers regard as the fundamental factors that determine the success of young graduates are arranged in sixteen groups in the following table in the order of their recurrence in the replies:

1. Character—integrity—personality.
2. Self-reliance—responsibility—perseverance.
3. Ability to understand and handle men.
4. Initiative—originality—resourcefulness.
5. Judgment—common sense.
6. Industry—energy—willingness to work.
7. Technical knowledge.
8. Scientific attitude—reasoning power—openmindedness.
9. Accuracy, care for detail—thoroughness.
10. Executive ability—efficiency.
11. Business training.
12. Breadth of view—general education.
13. Ambition—enthusiasm—interest.
14. Power of expression.
15. Morality—patience—modesty.
16. Application of theory to practice.

Since it is essential to the progress of engineering education that this definition of the profession's educational ideals be on a wide and representative basis, the committee brings this table to

the attention of the local engineering clubs with the request that they devote an evening to the discussion of it. Each member is then requested to return the enclosed card with the groups of qualities numbered in what he considers to be their order of importance.

Yours very truly,
C. R. MANN.

SAMPLE OF CARD TO BE SENT TO MEMBERS OF LOCAL ENGINEERING CLUBS

Please number the following in what you consider their order of importance for success in engineering:

Character.
Self-reliance.
Ability to understand men.
Initiative.
Judgment.
Industry.
Technical knowledge.
Scientific attitude.
Accuracy.
Executive ability.
Business training.
Breadth of view.
Ambition.
Power of expression.
Morality.
Application of theory to practice.

Name .....

Address .....

CHAIRMAN: Dr. Withrow showed me the tabulation showing the numbers of replies received from the previous questionnaire, which seemed very small. Probably the explanation is that the men were very busy and did not take time to look into it. They might again be sent out, along with a letter from the Secretary, in an effort to get a little fuller reply from the members. It is very desirable for this Institute to be recognized by the Carnegie

Foundation as taking an interest in the subject of education of chemical engineers. If we are chemical engineers we cannot shirk the responsibility of expressing opinions when required as to the status of the profession, what it ought to be, and in what respects it can be improved. It is very desirable that a second opportunity be given to have an expression from members who will take the trouble to answer a few questions.

Dr. WITHROW: I move that the request be made of the Carnegie Foundation that a second questionnaire be sent out. Carried.

REPORT OF THE COMMITTEE ON CHEMICAL ENGINEERING EDUCATION

Read at the Cleveland Meeting, June 14, 1916

THE AMERICAN INSTITUTE OF CHEMICAL ENGINEERS:
Gentlemen:

Definite progress has been made in the investigation of Engineering Education in general by the Carnegie Foundation for the Advancement of Teaching under the direction of Dr. Mann, since your committee's report at the Baltimore meeting of the Institute in January. This progress consisted in crystallizing the opinion of the members of the engineering profession with reference to the relative importance of the qualities which are believed to make for engineering success or are of greatest importance in sizing up young men for employment or promotion.

At the Baltimore meeting the Institute, by vote, authorized your committee to request the Carnegie Foundation to send to the membership of our Institute the circular letter questionnaire which the Foundation was contemplating sending throughout the country, to geographically local engineering clubs, to arouse discussion of the general subject of engineering education ideals. This request was accordingly made of the Foundation. It developed that others had suggested a similar procedure. The Foundation, therefore, on February 18th sent the second questionnaire to the individual members of the various Institutes of Engineering. In this questionnaire the Foundation was pleased to incorporate certain modifications suggested by your committee. Because of the paucity of the replies by the members of our Institute to the first Carnegie questionnaire, your chairman addressed a circular letter in the matter to the members January 15, 1916, just before the Carnegie second questionnaire. A second circular letter was formulated and the Secretary requested to send it to the members after the Foundation's second questionnaire, but this was not done. The Secretary felt that the Institute members were strongly replying to the second question-

naire. It is hoped that this opinion will prove correct. We have no data, however, at present.

Certain suggestions made to the Foundation with reference to some of the items in the second questionnaire were sent too late for consideration. They related to the qualities of initiative and resourcefulness and are discussed in the letter of February 21st in the Appendix to this report. The Foundation's second questionnaire under date of about February 18th was received a few days later.

The following copy of this second questionnaire letter explains itself:

Feb. 18, 1916.

Dear Sir:

Last spring we consulted you on the subject of engineering education, concerning the qualities that you look for in young engineers and the general effectiveness of the engineering schools in producing such men as you want.

The replies that have been received mention many factors that are essential to the successful engineer. These essential factors fall naturally into the following six groups which are named in the order of their importance as determined by their frequency of occurrence in the replies.

1. CHARACTER, integrity, responsibility, resourcefulness, initiative.
2. JUDGMENT, common sense, scientific attitude, perspective.
3. EFFICIENCY, thoroughness, accuracy, industry.
4. UNDERSTANDING OF MEN, executive ability.
5. KNOWLEDGE of the fundamentals of engineering science.
6. TECHNIQUE of practice and of business.

If this is a correct definition of the essential characteristics of an engineer, it furnishes the necessary basis for a rational discussion of the effectiveness of the schools. It can also be used as a standard in developing staff organizations and curricula, and in selecting efficient methods of instruction.

The best way in which engineers can aid the schools in solving problems in connection with engineering education is by defining the product which they desire. We therefore urge you to contribute to this important cause by numbering the six groups on the enclosed card in the order of importance which you give them in judging

the reasons for engineering success and in sizing up young men for employment or for promotion.

Yours very truly,

C. R. MANN.

P. S. We shall be glad to keep you informed of the progress of this study if you are interested.

Your committee's report would have ended at this point having done as required by the Institute's Baltimore meeting, but two days before leaving Columbus the Carnegie Foundation's digest of the replies to the second questionnaire was received. There has been no time to send it to the members of the committee or even call a meeting for this morning. The Carnegie Foundation's communication is therefore incorporated entire in this report of progress and is as follows:

June 5, 1916.

MY DEAR PROFESSOR WITHROW:

Enclosed I am sending you a preliminary statement of the summary of the returns from the second circular letter. I shall be glad to have your criticisms and suggestions concerning this statement.

I shall also be glad to have your vote on the following questions:

1. Shall I present this statement at the meeting of the Society for the Promotion of Engineering Education in Virginia on June 19?
2. Would it be desirable to present this preliminary report at the next meeting of the association which you represent?
3. Shall we distribute this to the society represented by publication in their respective journals?
4. Shall we send it to the Engineering News, the Engineering Record, and the Electrical World for general publication?
5. It has been suggested that the time is ripe to call a joint meeting of the societies some time next fall for the purpose of discussing engineering education and the status of the engineer. Does this seem to you desirable? Would your society cooperate in such an undertaking?

The Foundation is in favor of all of these propositions.

Yours very truly,

C. R. MANN.

Professor J. R. Withrow.

Preliminary Report on the Characteristics for Success in Engineering.

Last March the Joint Committee on Engineering Education sent to the members of the National Engineering Societies the following card:

Please prefix numbers to the groups of qualities listed below to show the order of importance that you give them in judging the reasons for engineering success or in sizing up young men for employment or promotion:

...Character, integrity, responsibility, resourcefulness, initiative.
...Judgment, common sense, scientific attitude, perspective.
...Efficiency, thoroughness, accuracy, industry.
...Understanding of men, executive ability.
...Knowledge of the fundamentals of engineering science.
...Technique of practice and of business.

Submitted by .....
Address .....

Years in practice.....

Up to June first, 6773 of these cards had been returned; 352 were accompanied by letters explaining the views of the writers.

A statistical summary of the returns was begun on May first, when 6069 cards had come in. Of these 5441 were regular, each one rating the six groups in the order of importance by some single combination of the numbers 1—6. There were 489 irregular cards, in which one or more of the numbers appeared more than once or in which the qualities were weighed by percentages instead of being numbered. On 139 cards the qualities were numbered in two or more different orders, one for engineering success, another for employment, etc. The regular cards were sorted according to the number of years in practice and the vote counted separately for each five-year period. Cards on which the years in practice were not given were counted together.

Differences in years in practice were found to make but little difference in the vote. The following table gives the regular vote by ten-year periods. The table reads: Character was rated higher than Judgment by 84.8 per cent of the voters who had less than ten

years of experience, by 86.3 per cent of those with from 10 to 19 years of experience, etc. The last column gives the percentage vote on each item for all the 5441 voters.

Yrs. in practice	Under 10	10–19	Over 20	Not given	Total
No. of ratings	1000	2000	1941	500	5441
C J	84.8	86.3	88.6	88.2	87.0
C E	92.5	92.5	92.8	91.8	92.5
C U	92.7	93.0	95.0	95.8	93.9
C K	91.8	91.0	91.9	91.4	91.5
C T	97.3	98.0	98.0	98.0	97.9
J E	77.8	78.7	76.3	75.4	77.4
J U	80.8	82.1	84.1	82.0	82.5
J K	78.4	79.7	74.7	75.8	77.4
J T	96.8	96.2	95.0	96.2	95.9
E U	59.7	61.5	68.5	65.4	64.0
E K	65.0	63.0	64.9	64.4	64.2
E T	93.9	92.9	92.7	95.8	93.4
U K	57.7	57.3	54.5	52.2	55.9
U T	89.9	87.1	85.7	86.0	87.0
K T	92.3	90.3	90.7	91.2	90.9

The most probable values of the relative importance of these groups of qualities have been computed from these percentage votes by the method described in Chapter VIII of Thorndike's Theory of Mental and Social Measurements. These values are given in the first column of figures. The last column gives these same values as determined by the summary of the replies to the first circular letter explained in the Engineering Record for January 29th and the Engineering News for January 27th, 1916.

Character	24.0		41.0	
Judgment	19.5		17.5	
Efficiency	16.5		14.5	
Understanding of men	15.0	75	14.0	87
Knowledge of fundamentals	15.0		7.0	
Technique	10.0	25	6.0	13
		100		100

It will be noted that the new determination reduces the importance of the character group from 41 per cent to 24 per cent, but increases that of all the rest. As a result, the relative importance of the first four groups of personal qualities has been reduced from 87 per cent to 75 per cent, and that of knowledge and technique has been increased from 13 per cent to 25 per cent.

Because of the constancy of the percentage of the votes, as shown in the table, it is clear that this statement of the factors essential to engineering success corresponds to a rather definite ideal in the professional mind. It is therefore safe to make use of this definition in testing and planning the work of the schools.

When applying this definition to the schools it is desirable not to forget several perfectly obvious facts. In the first place, all the qualities mentioned are essential to genuine success and conscious effort should be made to develop all of them as far as possible.

Second, character, initiative, common sense and qualities of this sort cannot be taught explicitly like multiplication tables and rules of grammar. Third, education is a continuous process of growth, and therefore the conscious development of the qualities of the first four groups cannot to advantage be arrested for four years, even for the sake of a mastery of knowledge and technique. Fourth, the man whose character, judgment, efficiency and understanding of men has developed most during his college years has the best show after graduation, since these qualities constitute 75 per cent of his equipment. First, the fact that character is rated 24 per cent does not mean that an engineer can succeed with a 24 per cent character. It does mean that even a perfect character is but 24 per cent of the engineer's total equipment.

With these facts in mind it is evident that a type of instruction in mathematics, or machine design which aims only at a mastery of fundamental principles and technique, cannot be nearly as efficient as one which, while paying due regard to technique and the mastery of principles, yet gives opportunity for the development of integrity, initiative, resourcefulness, and common sense.

C. R. MANN.

My individual vote as a member of the Joint Committee on Engineering Education was favorable on all five questions in the Foundation's letter of June 5th, except the last portion of the fifth question: "Would your society cooperate in such an undertaking?" Upon this I could express no opinion. It is a matter for the Institute to decide.

Respectfully submitted,

JAMES R. WITHROW,

Chairman.

APPENDIX TO REPORT.

January 25, 1916.

MY DEAR PROFESSOR WITHROW:

In accordance with the suggestions in your letters of December 2nd and 16th (see Bull. 12, Amer. Institute of Chemical Engineers, p. 13), we have revised the proposed circular letter and enclose a copy of it in its new form. It seems to me that we shall be more successful in securing reliable material from the engineers if we confine our efforts to the one point of trying to get them to define clearly and correctly what qualities they want the young men to possess when they receive them from the schools. Many of the questions which you suggest for discussion in your letter of December 2nd seem to me to belong essentially to the schools. I shall be glad to receive your comments on this new form of the circular letter at your convenience.

Yours very truly,

C. R. MANN

MEMORANDUM TO THE MEMBERS OF THE JOINT COMMITTEE.

Replies to the letter of December the thirteenth to the Joint Committee have been received from thirteen members. These express general agreement with the plan to return the summary of the engineers' letters to the engineers for further comment. Several valuable suggestions were made as to the form and the content of the proposed circular. In the light of these suggestions, it is now proposed to mail the enclosed revised circular to the members of the national engineering societies and to ask for a direct reply instead of trying to have it discussed in local societies.

The reasons for the changes are these: The grouping of the characteristics into six large but mutually exclusive groups makes the numbering of them in the order of importance much easier and less ambiguous.

It would be difficult to conduct a discussion on the order of importance of these groups, though every engineer can readily determine their order of importance for himself.

Questions concerning educational policies or practices are not added because the engineers will serve the schools best by stating clearly and definitely the qualities they would like to find in the young men delivered to them and then leaving to the schoolmen the problem of ways and means.

The problem before the committee is not that of determining whether the schools in general should be more vocational, should give less time to technical details and more time to humanities, etc. The answers to these questions may well be different in different localities. There are, however, certain fundamental characteristics that all engineers should have and that all schools should be expected to develop. A specification of these characteristics by the engineering profession defines the purpose of the school. It thus sets up a standard of achievement in the light of which each school can judge its own work and define its own policies. Is not the real problem before the committee that of drawing up this specification of ultimate purpose and of showing the schools what its implications are and how to use them?

January 15, 1916.

MEMBERS OF THE AMERICAN INSTITUTE OF CHEMICAL ENGINEERS:

Dear Sirs:

The Institute has always taken great pride in its work on Chemical Engineering Education. This work was actively carried on up until a year and a half ago when the Carnegie Foundation decided to investigate the teaching of engineering throughout the country. At that time we decided to await the results secured by the Foundation before proceeding further with our own studies. Since then the Institute has been made, at the Foundation's invitation, a member of their Advisory Board and in this connection the Foundation requested our Institute to send out a questionnaire last July. To this questionnaire our Institute has replied only to the extent of about 3 per cent of its membership, which gives us the poorest showing of any of the Institutes of Engineering in the country. This is particularly unfortunate in view of the fact that we have made the highest pretentions to interest in this matter. It, of course, rests with the individual members whether they will show the Carnegie Foundation that they are really interested in the subject of chemical engineering education to the extent we have professed to be in the past.

It has been stated by the Chairman of your Committee to the Foundation that the probable reason for this poor showing is a combination of the fact that the questionnaire was sent out during the summer vacation, with the additional fact that the stress produced in the chemical industries by the enforced production of munitions and other economic influences has thrown a heavy burden on the chemical engineers of the country. However, it behooves us to contribute the little time which will be necessary to answer the questionnaire so as to indicate our Institute's true interest in this important matter of Chemical Engineering Education.

The undersigned, therefore, requests the indulgence of the membership of the Institute to the extent that they hunt out the questionnaire, which was sent to them in July, answer it at once and forward it to the address thereon.

Thanking you for your attention, I am,

Sincerely yours, JAMES R. WITHROW,
Chairman of Committee on
Chemical Engineering Education.

February 21, 1916.

Dr. C. R. Mann,
Carnegie Foundation,
576 5th Ave., New York City.

Dear Sir:

Replying to your letter of January 25th in the matter of Engineering Education, I have now heard sufficiently from the enclosed letter to the members of the American Institute of Chemical Engineers which I had sent out January 15th so that I can reply to your letter of January 25th and ask certain things of you in line with the replies to my letter to our membership.

It appears from the replies that many of our members never received, or think they never received, the questionnaire which was sent out to them last July. Please let me know whether the Foundation itself cares to send out the questionnaire which you now propose to our membership. I believe it was done the other way before and sent out from the office of our Institute. It was probably sent out in good shape from the office of our Institute but was included in a letter with other mail matter to the membership as a circular and was overlooked on that account. If either of you send it out as a distinct proposition alone I believe the difficulty will be largely overcome. I may state that the Institute at its Baltimore meeting voted, at my request, to ask the Foundation for copies of the new questionnaire which we then understood was to go to local engineering clubs so that we could send it to our membership. We are glad, therefore, that you have proposed to change the original intention and sent the new questionnaire to the members of the various institutes, as this is exactly in line with the desires of the American Institute of Chemical Engineers.

I note in your comments (January 25th) on my letter of December 2nd that not so many of the questions which I suggested for discussion, as would appear on first sight, belong essentially to the schools as you have suggested, and therefore I find that your memorandum to the members of the Engineering societies covers very adequately the ground which I had in mind.

I am not so certain that I agree with you (though it is a minor matter) concerning the discussion of educational policies or practices by the practicing engineer. I am of the opinion that ways and means of carrying out educationally the ideals which, as you may rightly say, must be established by the profession, will profit

quite materially (as far as the University man is concerned) by the point of view of the professional man. In my own experience I note that while many of the suggestions made by the professional man are impracticable from an educational point of view and would usually be conceded by the professional man to be so, when he is informed of all the facts, nevertheless I always find much that is suggestive in the discussions by these professional men of the How and What to Teach.

In my own case I know that long before I ever thought I would be a teacher at all and with little idea of teaching industrial chemistry or chemical engineering, when engaged in professional work, I frequently thought how much better it would have been for me if I had taken industrial chemistry by other than the standard methods or so-called historical method usually pursued by the professors of this subject, and it naturally became a very interesting matter to me when I was offered the opportunity of teaching the subject to develop the ideas which I had formed years before, when engaged in professional work. One is naturally best pleased with his own ideas, but it has always made my work easier to accomplish because I had the advantage of this professional criticism on the methods of teaching this subject from an unbiased point of view, before I began teaching (even if the critic happened to be myself when I did not expect to teach the subject). I, however, see clearly the point which you make in the matter and do not suggest any change whatever in the plan which you have made, for it is simpler than complicating the situation with questions on educational policy or practices and I myself prefer it to be done as you have indicated.

With regard to the proposed letter to the members of the Engineering Societies, I would suggest, if feasible and in your opinion desirable, that Resourcefulness be withdrawn from group 1 under Character and placed either in group 2 (Judgment, etc.) when scientific capital, so to speak, is the prime factor, or in group 4 (Understanding of men, executive ability), where another kind of mental capacity or capital is the prime factor, for while resourcefulness may be considered as related to character, it is more directly related to the mentality indicated in 2 or 4, and particularly the former, for resourcefulness, like judgment, hinges largely on experience. I would suggest also that Initiative be removed from group 1 under Character, for while it is related somewhat closely

to character, yet it is more related to the group 3, which is headed Efficiency, etc., and I would place it first before Efficiency in group 3.

Hoping that these suggestions may be of service to you and that I may hear from you in the matter of the methods of sending out the questionnaire to our Institute, I am,

Sincerely,

JAMES R. WITHROW.

P. S. It might be possible for you to add the items *Capacity for Scientific Thinking in number 2 and in number 6 or number 5, Ability to Utilize Theory in Practice,* but I am not insistent on these things, though I think they are very important.

J. R. W.

CHAIRMAN: I presume this is a report of progress and it is still unfinished, I take it?

Prof. WITHROW: Yes.

CHAIRMAN: Shall we accept the report of Prof. Withrow with the understanding that it is a report of progress? Is there a motion to adopt the report?

It was moved and seconded that Prof. Withrow's report be adopted as read. Motion carried.

SECRETARY: There was one matter that was brought out in the report that I presume should come before the Institute, that is, whether we should appoint a representative to meet with the Carnegie people in New York on this matter.

Prof. WITHROW: I understand it is their intention that the whole society meet and hold a general meeting with other engineering societies for the discussion of this topic,—that appears to be their suggestion and that is why I read it in full. They have been working for two years and feel they have gotten far enough along to bring these reports before the engineering societies to get a joint discussion. That is the suggestion they make and we can vote either aye or nay on this proposition.

SECRETARY: In this connection I think one or two things should be presented to the Institute so you will understand the situation.

There will be held in New York this fall the second industrial exhibit of the chemical industries, and at that time we will be again asked whether we wish to co-operate. We had a joint session last fall and that was more or less successful. We can enter it

again without any detriment, I imagine, and possibly make it more successful.

Then again, I want at this time to bring to your attention a report of a Committee on University and Industry which was presented at a meeting of the New York section of the American Chemical Society, June 9th. I was a member of the committee of nine, five of whom are members of this Institute who collaborated in this report.

Now, it is the intention to bring about in the Fall the appointment of a permanent committee upon which there shall be representatives from the industries and representatives from the schools or universities, and I imagine that this committee will take up in a particular way not only education but co-operation as far as research work is concerned, and, in general, bring about a closer relation between the industries and the schools which are producing the technical men. This is going to be brought up in the American Chemical Society; it has not been officially presented to the American Institute of Chemical Engineers, but I think this society is interested.

I personally am of the opinion that we can say yes to this request of the Carnegie Foundation to join in holding a joint meeting between all the engineering societies. I think in the first place we should certainly be represented. We have a representative on the committee and if we have such a joint session there are plenty of members in New York and in the vicinity to attend the meeting; and when it comes to a question of being represented on the program, I am sure that the chairman of our committee is as competent as any man that could be put up by any other society, and he is so interested I believe he would be present if necessary.

Prof. Withrow: I make a motion that it is the sentiment of this meeting that we hold such a joint meeting as the Carnegie Foundation suggests if it can be arranged by the officers of the Institute.

Dr. Andrews: It occurs to me in this connection that it is worth while to consider whether this society might not at such a joint meeting be more adequately represented through delegates specially appointed for that purpose than if all the members are simply invited to go to a general meeting. The conditions are such now and probably will be in the Fall in the chemical industries that it is very difficult for our men to get away now; and to get

away a second time in the Fall and to a meeting of that character would be particularly difficult, so that the attendance could be small. Whereas it might be possible for us to pick out three or four delegates, who, with that feeling of responsibility placed upon them by our Society, would make a point of it and would represent us more adequately than if we left the selection of those men who are to go there to accident.

Prof. WITHROW: I accept that amendment.

CHAIRMAN: I take it Dr. Mann's original request was for representatives.

Prof. WITHROW: This is an addition. My motion was that we accede to have the joint meeting; this is an additional suggestion that we not only hold the meeting but be officially represented.

Dr. ANDREWS: I will put that in the form of an amendment: That the President appoint four delegates from this Society to represent this Society and meet with the joint committee in New York City in the Fall.

Prof. WITHROW: It is, of course, understood that that does not mean that just these four delegates attend the meeting.

Motion of Dr. Andrews duly seconded and carried.

CHAIRMAN: I suppose it isn't necessary to vote on the motion as amended; we will pass that by and assume that, without objection, it is so ordered.

REPORT OF THE COMMITTEE ON CHEMICAL ENGINEERING EDUCATION

Read at the New York Meeting, January 10, 1917

THE AMERICAN INSTITUTE OF CHEMICAL ENGINEERS:

Gentlemen:

Since the report to the Institute at its Cleveland Meeting still further progress has been made by the Carnegie Foundation for the Advancement of Teaching in its campaign on engineering education and matters have been actively followed by the Institute representative on the advisory board of the Carnegie Foundation's Engineering work (the Chairman of this Committee).

As it has been decided by the Institute to await the outcome of the Carnegie Foundation's efforts in the general field of engineering education, your Committee has held no meeting since the last report and desires at this time to merely report the activities of the Carnegie Foundation with comments thereon.

Your representative on the Advisory Council for Engineering Education of the Carnegie Foundation by invitation of the Society for the Promotion of Engineering Education attended the meeting of that organization at the University of Virginia, Charlottesville, Va., late in June, 1916, at which meeting the first extended report of progress was made by Dr. C. R. Mann of the Carnegie Foundation's Engineering Education work. Your representative also called upon Dr. Mann at the New York office of the Foundation late in September, 1916, for discussion of the Foundation's plan for a joint meeting of the engineering societies.

The Carnegie Foundation's efforts seem to be planned to act as stimulation to the individual teachers in the engineering schools and colleges. In addition it is laying a foundation for building an engineering ideal for both teachers and the profession.

Dr. Mann's report at the meeting of the S. P. E. E. was devoted to

1. Historical Background of American Engineering.
2. The Professional Demand.

3. The Work of the Schools in the Fundamental or Elementary Subjects.

In a former report we called attention to the fact that Dr. Mann in his personal survey of the engineering schools of the country concluded that on the whole technical teaching was well handled and that the best service could be rendered by the Foundation in the Fundamental subjects and in correlating these with the advanced technical ones.

The Institute voted at Cleveland to co-operate in every way possible with the Foundation in its project to hold a joint meeting of all the engineering societies for concerted action where the Foundation felt such desirable. The Foundation was notified of our action. Your representative on the Joint Committee visited Dr. Mann in New York to arrange details and was assured of the thanks of the Foundation for our willingness to co-operate, and informed that the matter would be held in abeyance until the Report of Progress mentioned above had been given adequate publicity among the profession. Your representative was asked for suggestions and criticisms of the Report of Progress and for a discussion by the S. P. E. E.* and these have been forwarded. The President and Secretary of the S. P. E. E. promised a copy of discussion of the report held at a meeting in the absence of your representative but it has not been received nor has it as yet been published.

Therefore, for the information of our members and also to assist the Carnegie Foundation for the Advancement of Teaching in its desire for publicity and to form a permanent record of progress in one of the fundamental ideals of our Institute we recommend, that this report and the appended "Report of Progress in the Study of Engineering Education" by the Carnegie Foundation with comments thereon by the representative of our Institute be published in our Transactions.

Columbus, Ohio,
January 1, 1917.

JAMES R. WITHROW, Chairman.
SAM. P. SADTLER.
M. C. WHITAKER.
A. D. LITTLE.

* Society for the Promotion of Engineering Education.

APPENDIX TO THE ABOVE REPORT.

(Read by Professor Mann at the University of Virginia Meeting of the S. P. E. E., June, 1916)

REPORT OF PROGRESS IN THE STUDY OF ENGINEERING EDUCATION.

By C. R. Mann.

The chairman of this Committee and others have expressed their confidence that a great deal of good will come from this study. This seems probable for two reasons. One is that it is being guided by so able and so devoted a committee. The other is that the schools have welcomed the study and shown a hearty willingness to cooperate in every possible way. Under such conditions, how can anything but good result?

Last year it was reported that the study was being carried on under four heads:

First, a historical study, for the purpose of defining the meaning of the words "American engineer."

Second, a study of the professional demand, to determine what it is that the professional engineer expects the school to achieve.

Third, a study of the schools in detail, with an analysis of their work with reference to the demands of the engineering profession.

Fourth, an investigation into objective methods of testing school work;—of finding out whether men are competent to enter the engineering schools; and how effective the work is when measured by its results.

1. The Historical Background.

The basis of the American system of education was laid by the general court of Massachusetts in two acts passed in 1642 and 1647, respectively. These state that the aim of education should be to secure four concrete ends of great value to the church and the commonwealth. These ends were, for the church, that every one must (1) learn to read the Scriptures and the catechism, and (2) have the free opportunity of entering the ministry through the

grammar school and the college; for the commonwealth, that every citizen should learn (3) the capital laws of the colony, and (4) some gainful occupation.

At the time that this legislation was enacted the only occupations open to graduates of the college were those of minister, teacher and gentleman. The great majority of the people, including physicians and lawyers, learned their trades by the apprenticeship system. Therefore the responsibility for their education was divided between the schoolmaster and the master of apprentices. The schoolmaster was "to teach all such children as may resort to him to write and reade"; and be "able to instruct youth so farr as they may be fited for the university." The master of apprentices was to train them "in some honest lawful calling, labour or imployment, either in husbandry or some other trade profitable for themselves and the Commonwealth."

This division of the function of education between the schoolmasters and the masters of apprentices was inevitable under the social and industrial conditions prevalent in the colonies. In time, however, schools came to be regarded as constituting the whole educational system, and the fact that the training of every one to some "gainful occupation" is one of the important justifications of taxation for public education was forgotten. The magnificent service which the schools have rendered in conserving ideals in America is fully described in the standard histories of education. But the industries and mechanic arts, which have rendered a no less magnificent service in expressing American ideals and developing the American spirit, have received but scant recognition as essential elements of the national system of education. Since the industries and the mechanic arts have laid the material foundations of the United States and created the demand for engineers, it is not possible to understand engineering education without at least a brief sketch of the development of the demand that training in the practical arts be reinstated as one of the four essentials of a complete education.

The industrial history of America begins even before that of the schools. The first ship built by white men on this side of the Atlantic was launched in 1607 at the mouth of the Kennebec. A tannery was established at Lynn in 1629. In 1640 the General Court of Massachusetts directed the magistrates to consider "what course may be taken for teaching the boys and girls in all townes

the spinning of yarn." By 1690 the American weavers of woolen cloth had become such successful rivals of the British weavers that Parliament passed the Woolens Act which forbade the colonists from transporting woolen goods from one place to another for the purpose of sale.

"In 1718 a great stir was created in the town (Boston) by the arrival of a number of Irish spinners and weavers, bringing the implements of their craft. Directly the spinning craze took possession of the town and the women, young and old, high and low, rich and poor, flocked into the spinning school which was set up on the common in the open air. Prizes were offered for the best work and the enthusiasts went about proudly clothed in the homespun products of their own hands."

A special town meeting was held at the town house September 28, 1720, at which it was voted "that the Town will proceed to the choyce of a committee to consider about promoting of a Spinning School or schools for the instruction of the children of this town in Spinning." This committee recommended the erection of a suitable house and the employment of a weaver "having a wife that can instruct children in spinning flax, to take care of the school." This project was not carried out till 1753 when a wheel tax was levied, £1,500 raised, and the Manufactory House built for this purpose. In 1765 the Daughters of Liberty resolved to wear only homespun, and the seniors of Harvard College agreed to take their degrees (1768) "dressed altogether in the manufactures of this country."

It is a well-recognized fact that the efforts of the British to crush American manufacturing industries were among the chief causes of the Revolutionary War. The only positive action of the first continental congress (1774) was its non-importation agreement, aimed at establishing independence in productive industry. After the Peace of Paris, under the confederation, each colony controlled its own trade. Because there was thus no concerted action with regard to industrial protection, England was able to flood the American markets with foreign goods which were sold at prices with which home manufacturers could not compete. American industry was paralyzed, money became scarce in America, and the workingmen were idle.

To meet this situation, associations of mechanics and tradesmen were organized in various parts of the country. These took an active part in the struggle for the ratification of the constitution in

the ardent hope that a federal government would be able to protect and encourage American manufactures. In this they were not disappointed, for the first act of the consolidated government (1789) was a statute for the joint purposes of "raising revenue and protecting manufactures by laying duties on goods, wares and merchandise imported." The first federal patent law was passed in 1790.

Not only were the manufacturers protected from foreign competition by the federal tariff, but they were also encouraged by financial aid from state legislatures. A bill levying taxes for the support of associations for the promotion of agriculture, manufactures and the useful arts was introduced into the Pennsylvania state legislature in 1798. Other states followed suit, but the first to vote funds for this purpose was New York. In 1819 an appropriation of $10,000 a year for two years was made. This state support led to the establishment of twenty-nine county societies for the promotion of agriculture and domestic manufactures during the two years.

This protection of American industry by the federal tariff and by state support seemed necessary at this time because the use of machinery and the factory system had progressed further abroad than it had in this country. In order to maintain this advantage, Parliament had forbidden the export of machinery to America. In 1788, however, Samuel Slater, a mechanic who had had experience with the British machines escaped to America and built from memory similar machines here. Under his direction the first successful cotton mill driven by water power was established at Pawtucket in 1790.

The invention of the cotton gin by Eli Whitney in 1792, the application of steam power to navigation by John Fitch (1790) and Robert Fulton (1808), the development of machinery for threshing grain and milling flour by Oliver Evans (1791–1804) gave great impetus to these several industries, and domestic manufactures advanced by leaps and bounds.

Great difficulty was experienced in obtaining operatives as the manufacturers were "required to raise up an entire new set of workmen from amongst our own Youth at a heavy expence." These operatives were poor children seven to twelve years old whose parents had been able to give them small opportunities for schooling. Therefore, in 1793, Slater established at his own expense a "Sunday School" where these children were taught the three R's

by students from Rhode Island College. These Sunday Schools were the precursors of the mechanics' institutes.

As the manufacturing industries developed and machinery came to be used more and more, the difficulty of securing skilled labor increased. Signs of exhaustion of the soil also began to make it evident that more intensive and scientific methods of farming must be introduced. In 1818 there was published in the *Gentleman's Magazine* the first account in English of the Fellenberg industrial school at Hofwyl, Switzerland. The Hofwyl school was first established as a charitable enterprise for the education of poor children. Its work was such a marked success that the children of the rich soon sought admission too. Its aim was the development of character by all round training. Careful attention was given to physical health and to recreation by change of employment. Time was judiciously divided between books and the work of caring for the place. Sound morals were inculcated through good books, good environment, self-government, no prizes, and mutual confidence among instructors and students. The plan and methods of this school seemed so well suited to the needs of the situation in America that a similar school was established the next year by Josiah Holbrook at Derby, Connecticut.

This school was not a success, but the idea spread; and in 1823 the Committee on Agriculture of the New York Legislature made a report in which the establishment of a state school of agriculture was urged. The institution was to be modeled after the Fellenberg school to consist of (1) a model farm; (2) an experimental farm; (3) a manufactory of implements of husbandry; (4) a school of industry and mechanic arts; (5) a boarding school; and (6) an institute of theoretical and practical agriculture. Arguments were adduced to show why such a school would benefit agriculture, commerce, manufacturers, the morals of society, the revenues of the state, and politics.

This report closes with the remark "The Hon. Stephen van Rensselaer has offered a gratuitous deed of the lands required for the use of the institution." The legislature failed to adopt the report and the next year van Rensselaer proceeded to carry out the project alone and founded the Rensselaer Polytechnic Institute, the first engineering school in the United States. The methods of instruction introduced into this school by Amos Eaton, its first direc-

tor, were original and somewhat different from those of the Fellenberg school.

At this same time the public schools reached, according to the histories of education, the low water mark of their efficiency.

The year 1824 witnessed the inauguration of another enterprise which has been of far reaching usefulness to technical education. Samuel V. Merrick inherited that year a machine shop in Philadelphia. He soon discovered that he could not manage it intelligently without some knowledge of machinery. Although the University of Pennsylvania had in 1816 "created a new department to be devoted to the study of natural science," and although five professors were still holding chairs in that department, Merrick was unable to get the kind of instruction he needed. He therefore decided to establish a new school. Professor Keating of the university department of applied chemistry joined in the venture. This combination of Merrick's need with Keating's knowledge proved effective. The Franklin Institute was established. One of its first students, a bricklayer named Thomas U. Walter, designed the dome of the Capitol at Washington.

From 1825 to 1860 the industries developed rapidly. Canals and railroads were built, the McCormick reaper, the sewing machine and the telegraph were invented, and the Atlantic cables were laid. The west was settled, the federal finances prospered, and state school systems were developed. Every new stage of advancement in material prosperity and every new invention served to intensify the demand for greater knowledge of science and greater skill in the useful arts. This national demand had become so insistent by 1857, that it could no longer be ignored. The Morrill bill was introduced into Congress, and became a law in 1862.

It is important to note that this response to the national demand for practical knowledge came from the federal government. State school systems had been established by each state independently and on traditional lines; but this act calls for a new American type of school "to promote the liberal and practical education of the industrial classes in the several pursuits and professions in life." It is also significant that the father of this land grant college bill was father of the tariff act of 1861, and that bills increasing the federal bounties to these colleges were introduced into Congress during the tariff discussions in 1872, 1884 and 1890.

The decade from 1860 to 1870 witnessed the establishment of

several of the most celebrated engineering schools. In following their subsequent development it is important to recognize clearly that their origin was very different from that of the schools of law and medicine. These latter were first established by practitioners as an outgrowth of the apprenticeship system and were usually well developed as professional schools before they became affiliated with the colleges. The engineering schools, on the other hand, were not founded by engineers as the outgrowth of an apprenticeship system, but by college professors who sought to satisfy industrial needs by the methods to which they were accustomed in the colleges.

Yet notwithstanding this close kinship of the engineering school to the arts college, the scientific school was kept distinct from the "college proper." The students preparing for an industrial profession were not considered as of the same caste with those preparing for the "learned professions."

This feeling in the colleges was reflected in the public schools which were still oblivious to the fact that training for a "gainful occupation" is one of the four fundamental justifications of taxation for public education. The lowest depths of this oblivion were reached in the report of the Committee of Ten (1895) when languages, mathematics, history, natural history, physics, and chemistry, were declared to be the only subjects "proper for secondary schools."

The achievements of engineering have, however, forced the recognition of the fact that knowledge of the practical arts is essential to the education of every American. The demand for vocational education is omnipresent, and the schools seem to be about ready to follow the lead of the engineering colleges in so teaching the mechanic arts that their educative power shall be felt by everyone. Since the apprenticeship system has now practically disappeared, this introduction of the industrial arts into school work is necessary before the schools can justly claim to give a complete education, *i. e.*, one that confers all the benefits on the church and commonwealth which the founders of the American educational system had in view.

For the past fifty year engineers have been absorbed in the work of inventing, constructing, and perfecting machinery and the material conveniences of life. A large field for usefulness along these lines will always remain open to them. But the methods of this work have now been standardized and reduced to a system.

Every large plant has its designing department and even its research laboratory.

Relatively little attention has as yet been given to the scientific study of problems of the organization and control of the forces of men. These problems of human machine design and the conservation of human resources are as much engineering problems as are those of mechanism and the conservation of material resources. The demand for creative work in this field of human engineering is daily becoming more insistent. Can the engineering schools longer afford to ignore it?

Again, American industry has developed in a land of abundant natural resources. It has always been protected by a tariff which has been raised as the industries grew stronger and more wealthy. This protected atmosphere of natural opulence has given strong incentive for the development of originality, initiative, and push. Every boy in America has felt that the opportunity to become president was open before him. There was, however, little incentive for economy of production. It was much easier to exploit the Indians or to persuade Congress to raise the tariff than it was to make a scientific study of production costs.

The increase in population and the waste of natural resources have now brought the United States under the operation of the law of diminishing returns. The problem of efficiency of production has been thrust upon us. This again is an engineering problem. It cannot be solved here by merely copying the autocratic methods that have been used successfully elsewhere, but demands an American solution which shall insure efficiency without sacrificing individual spontaneity and initiative.

These then are the three problems of the immediate future for the American engineer. (1) To introduce the practical arts into the schools in such a way that all may share their educational value. (2) To devise human machinery that shall assure to every American justice and a fair show. (3) To develop the highest efficiency of production,—an efficiency which is attainable only when individual initiative is given the freest play.

II. The Professional Demand.

The returns from the circular letter sent out in the spring of 1915 were summarized and the results announced at the meeting of the American Society of Civil Engineers last January. Because

of the rather unexpected nature of the summary and its importance in defining the work of the schools, the Joint Committee on Engineering Education last March sent to the members of the National Engineering Societies the following card:

Please prefix numbers to the groups of qualities listed below to show the order of importance that you give them in judging the reasons for engineering success or in sizing up young men for employment or promotion.

...Character, integrity, responsibility, resourcefulness, initiative.
...Judgment, common sense, scientific attitude, perspective.
...Efficiency, thoroughness, accuracy, industry.
...Understanding of men, executive ability.
...Knowledge of the fundamentals of engineering science.
...Technique of practice and of business.

Submitted by ....
Address ....

Years in practice ....

Up to June first, 6773 of these cards had been returned; 352 were accompanied by letters explaining the views of the writers.

A statistical summary of the returns was begun on May first, when 6069 cards had come in. Of these 5441 were regular, each one rating the six groups in the order of importance by some single combination of the numbers 1–6. There were 489 irregular cards, in which one or more of the numbers appeared more than once or in which the qualities were weighted by percentages instead of being numbered. On 139 cards the qualities were numbered in two or more different orders, one for engineering success, another for employment, etc. The regular cards were sorted according to the number of years in practice and the vote counted separately for each five-year period. Cards on which the years in practice were not given were counted together.

Differences in years of practice were found to make but little difference in the vote. The following table gives the regular vote by ten-year periods. The table reads: Character was rated higher than judgment by 84.8 per cent of the voters who had less than ten years of experience, by 86.6 per cent of those with from ten to nineteen years of experience, etc. The last column gives the percentage vote on each item for all the 5441 voters.

	Years in Practice.				
	Under 10.	10–19.	Over 20.	Not Given.	Total.
No. of ratings	1000	2000	1941	500	5441
C>*J*	84.8	86.6	88.3	88.2	87.0
C>*E*	92.5	92.8	92.5	91.8	92.5
C>*U*	92.7	93.0	95.0	95.8	93.9
C>*K*	91.8	91.9	91.0	91.4	91.5
C>*T*	97.3	98.0	98.0	98.0	97.9
J>*E*	77.8	78.7	76.3	75.4	77.4
J>*U*	80.8	82.1	84.1	82.0	82.5
J>*K*	78.4	79.7	74.7	75.8	77.4
J>*T*	96.8	96.2	95.0	96.2	95.9
E>*U*	59 7	61.5	68.5	65.4	64.0
E>*K*	65.0	63.0	64.9	64.4	64.2
E>*T*	93.9	92.9	92.7	95.8	93 4
U>*K*	57.7	57.3	54.5	52.2	55.9
U>*T*	89.9	87.1	85.7	86.0	87.0
K>*T*	92.3	90.3	90.7	91.2	90.9

The most probable values of the relative importance of these groups of qualities have been computed from these percentage votes by the method described in Chapter VIII of Thorndike's "Theory of Mental and Social Measurements." These values are given in the first column of figures. The last column gives these same values as determined by the summary of the replies to the first circular letter explained in the *Engineering Record* for January 29 and the *Engineering News* for January 27, 1916.

Character	24.0		41.0	
Judgment	19.5		17.5	
Efficiency	16.5		14.5	
Understanding of men	15.0	75	14.0	87
Knowledge of fundamentals	15.0		7.0	
Technique	10.0	25	6.0	13
		100		100

Because of the constancy of the percentage of the votes, as

shown in the table, it is clear that this statement of the factors essential to engineering success corresponds to a rather definite ideal in the professional mind. It is therefore safe to make use of this definition in testing and planning the work of the schools.

When applying this definition to the schools it is desirable not to forget several perfectly obvious facts. In the first place, all the qualities mentioned are essential to genuine success and conscious effort should be made to develop all of them as far as is possible. Second, character, initiative, common sense, and qualities of this sort cannot be taught explicitly like multiplication tables and rules of grammar. Third, education is a continuous process of growth, and therefore the conscious development of the qualities of the first four groups cannot to advantage be arrested for four years, even for the sake of a mastery of knowledge and technique. Fourth, the man whose character, judgment, efficiency and understanding of men has developed most during his college years has the best show after graduation, since these qualities constitute 75 per cent of his equipment. Fifth, the fact that character is rated at 24 per cent does not mean that an engineer can succeed with a 24 per cent character. It does mean that even a perfect character is but 24 per cent of the engineer's total equipment.

With these facts in mind it is evident that instruction in mathematics, or machine design which aims only at a mastery of fundamental principles and technique, cannot be nearly as efficient as instruction which, while paying due regard to technique and the mastery of principles, yet develops integrity, initiative, resourcefulness, and common sense.

III. The Work of the Schools.

A. What Freshmen Know and Can Do.

Twenty-two schools have been visited and carefully studied. A composite picture of their work furnishes the best statement of conditions. Tests of 220 freshmen at four institutions have been conducted by Professor E. L. Thorndike. These tests furnish objectively certain facts about the human material which schools of engineering educate, showing what the members of the entering

class know and can do. The following samples may serve to give some idea of their abilities.

In Algebra, nine-tenths of the freshmen were able to solve the following equation: $x-2a+b=2x+2b-4a$. Only one-third of them could solve the following: If $x=(a+b)/2$, what does

$$(x-a)^3/(x-b)-(x-2a+b)/(x+a-2b)$$

equal?

In Physics, nine-tenths were able to tell what materials were needed to make an electric battery. Only one-third could determine how much water must be added to a pint of alcohol 95 per cent pure to make a solution of alcohol 40 per cent pure.

Nine-tenths were able to fill the blanks in the following: Light generally travels in .... lines. 100 Centigrade degrees equal .... Fahrenheit degrees. Only one-third could correctly supply the missing words in: The mechanical advantage of a system of pulleys, provided no energy is dissipated inside the pulleys, is .... the number of .... of the cord supporting the load. When a mass of air is heated ...., its volume increases .... of its value at zero degrees for every rise of one degree C. in temperature.

English.—In English, nine-tenths could classify as animal, flower, boy's name, book, something good or bad to be or do, the following words: zachary, dextrous, hyacinth, clematis, callous, mastodon, caribou, julian, ruthless.

Only one-third could similarly classify: predatory, breviary, sophistical, campanula, pusillanimous.

Nine-tenths could fill in "A body of ..... entirely surrounded by ..... is called an ....."

Only one-third were able to make sense out of "Let us briefly examine the social forces ..... at work concentrating or ..... the ownership ..... wealth."

Nine-tenths could write the answers to the questions on the following paragraph:

"It may seem at first thought that every boy and girl who goes to school ought to do all the work that the teacher wishes done. But sometimes other duties prevent even the best boy or girl from doing so. If a boy's or girl's father died and he had to work afternoons and evenings to earn money to help his mother, such might be the case. A good girl might let her lessons go undone in order to help her mother by taking care of the baby."

1. What is it that might seem at first thought to be true, but really is false? .....

2. What might be the effect of his father's death upon the way a boy spent his time?

3. Who is mentioned in the paragraph as the person who desires to have all lessons completely done?

Only one-third could answer the questions on the following:

"A chief characteristic of science, then, is that in supplementing given facts it supplements them by adding other facts belonging to the same sphere, and eventually discoverable by tracing the given object in its own plane through its continuous transformations. Science expands speculatively, by the aid of merely instrumental hypotheses, objects given in perception until they compose a congruous, self-supporting world, all parts of which might be observed consecutively. What a scientific hypothesis interpolates among the given facts—the atomic structure of things, for instance—might come in time under the direct fire of attention, fixed more scrupulously, longer, or with better instruments upon those facts themselves. Otherwise the hypothesis that assumed that structure would be simply false, just as a hypothesis that the interior of the earth is full of molten fire would be false, if on inspection nothing were found there but solid rock. Science does not merely prolong a habit of inference: it verifies and solves the inference by reaching the fact inferred. The contrast with myth at this point is very interesting; for in myth the facts are themselves made vehicles, and knowledge is felt to terminate in an independent existence on a higher or deeper level than any immediate fact; and in this circumstance is what makes myth impossible to verify, and, except by laughter, to disprove."

1. According to the paragraph, what is the difference between the ether, the electrons or lines of electro-magnetic force and angels, a sun-god, or fairies, as principles of explanation?

2. According to the paragraph, by what sort of explanation of the facts of the world are the facts themselves made mere carriers of thought to conclusions in a realm outside that of such facts?

3. According to the paragraph, what are the methods and instruments by the aid of which perceived things and qualities are made the basis of a consistent independent universe?

B. What the Schools do to Freshmen.

Two thousand freshmen of the type described entered a group of schools in the fall of 1911. Of these 732 graduated in June, 1915. The other 1268 had been "weeded out."

The records of the 732 who graduated from the 14 different schools showed that 382, or 52.2 per cent, just "got by" in physics, 52 per cent just passed in mechanics, 45 per cent in calculus, 43.7 per cent in modern languages, 43 per cent in English, 34 per cent in chemistry, and 23 per cent in descriptive geometry. These averages for 14 schools do not differ materially from those presented last year for five schools.

There are many who maintain that these conditions are most encouraging; for the fact that only one-sixth of those who enter graduate with credit proves that the schools are maintaining high standards. This position would be more readily understood by the critics of the schools, who constitute the great majority of the American public, if the term standards were more clearly defined. What sort of standards are being maintained by this process? Are they standards of scholarship, standards of academic culture, standards of engineering efficiency, standards of justice, of integrity, of morality, of common sense, of accuracy, of science, of art, of freedom, of achievement, of worldy success, of unity, of service, of obedience, of duty, of law and order, of aristocracy, of democracy, or of humanity? Perhaps all of these and many more are meant. It would be real service if the exact meaning of the phrase "maintaining standards" in schools might be accurately defined.

On the other hand those who consider that this high scholastic mortality and these high percentages of low grades are proofs of the inefficiency of the schools offer many hypotheses in support of their position. Among these two are particularly suggestive. The first of these is derived by analogy from the experience of the industries with "soldiering." Under the old industrial system, workmen are paid according to the number of hours of work, and little attention is given to the amount accomplished per hour. Under this system the workman accomplishes as little as he dares and unions are formed to raise the time rate and to standardize this minimum accomplishment. When the reward is measured solely by time and not by achievement, there is every incentive not only for laziness but even for deliberate deception of the employer as to

how much can be accomplished per hour by a willing and properly instructed workman.

The school system is analogous to this old industrial system since it awards "credits" in accordance with Commissioner Finley's "table of academic weights and measures," namely:

45 minutes make 1 hour
5 hours make 1 week
36 weeks make 1 unit
15 units make 1 matriculant
120 semester-hours make one degree

The student who puts in the requisite time with an estimated efficiency of only 60 per cent receives the same "credit" as one who achieves an efficiency of 100 per cent. The result is similar to that in the industries—many students "soldier," try to bluff their way through and even unite to create the tradition that it is a disgrace to be rated much above the pass mark.

In the industries this situation has been met first, by making a careful, detailed, scientific study of all the conditions that affect efficiency; second, by instructing the workmen in the best methods of work; and third, by making the reward depend on accomplishment. Where the old industrial system has been replaced by this new system, in accordance with the principles enunciated by Mr. F. W. Taylor, soldiering has disappeared, the workmen have doubled or even quadrupled their achievement, their earnings have increased, and they have become more contented and more healthy.

The success of this change of policy in industry makes us wonder whether an analogous change might not prove equally beneficial to the schools.

Usually the method of presenting subject matter in schools is determined by the teacher's conception of logical rigor and theoretical unity. In spite of the fact that the high percentages of failure and low grades prove that this method is not effective, very few experiments have as yet been made to find methods better adapted to the abilities of the students. But if the same scientific care were used in setting tasks in schools as is used in setting tasks in industry, would not these conditions improve rapidly? And would not school grades be a truer measure of ability if tasks were graded from easy to difficult and credits were given as is done in industry only for those actually completed?

Is it not probable that this system of awarding credit on the basis of achievement rather than on the basis of time would give to students a real incentive for real accomplishment as it has to workmen?

Industrial managers not only spend hours and days of study in adapting the task to the workman; they take great pains to see that each worker is carefully instructed in the most efficient methods of doing his work. May it not be that more time could be spent to advantage in instructing students in the most efficient methods of doing their work, instead of leaving them to devise their own methods of study?

Thus the first hypothesis that is suggested to explain the high mortality and excessive percentages of low grades is that the method of awarding credit on the basis of time, the acceptance of a 60 per cent standard of accomplishment and the assignment of tasks that are not adapted to his abilities gives the student no adequate incentive to achievement and encourages him in "soldiering" and in bluffing his way through.

Soldiering alone does not explain all the cases of failure and low grades. Many of those who are dropped from engineering schools should be dropped because they have no real inclination or ability for engineering work. A number of those dropped, however, persist in engineering work and succeed in spite of their academic shortcomings. Why are so many young men with pronounced engineering talent unable to make good in the engineering schools?

In one school some 74 per cent of the graduates had had trouble in mechanics. The work of the class consisted in the solution of many problems—apparently an excellent plan. But the first problems of the course were these: Find the center of gravity of an equilateral triangle. Find the center of gravity of the area included between three sides of a square and the semicircle described on the fourth side as a diameter. Find the center of gravity of the area bounded by a given hyperbola, its asymptotes and given coordinates. No models were supplied.

The use of these strictly Archimedean problems was defended on the ground that the analytical processes involved in their solution were those needed the following term in the solution of problems in strength of girders. Yet 74 per cent of these same students had trouble with this later course.

Why not give the real problems first? In the University of Washington, the course in mechanics, given by Professor C. C. More, is made up almost entirely of a series of real problems or projects of the grade usually found in such courses. This work was originally given in the third year because calculus was a prerequisite. The third-year men have, however, mastered it so readily that next year it will be given, calculus and all, to the freshmen. In like manner Professor Bird, of the University of Virginia, finds that laboratory problems very similar to those given in other schools, to seniors in chemical engineering can be successfully mastered by freshmen.

These examples define the second hypothesis that is suggested to explain the high percentages of failure and of low grades; namely, the work is not too hard, but it is presented in so abstract a manner that it has little or no meaning to the students.

If these hypotheses are correct, the problem before the schools is that of removing these two causes of failure. To do this, academic credit must be made to depend directly and objectively on achievement; and the work must be made more concrete.

This statement has been derived from an analysis of the work of the schools. Yet the problems which it defines are the same as those defined by the history of the engineering schools. For when the practical arts are introduced into the schools in such a way that all may share their educational value, the work becomes concrete and impelling to the student. And when academic credit is awarded on the basis of real achievement, efficiency and individual initiative increase together.

The solution of these problems will require much careful experimenting and study on the part of teachers; but as progress is made the schools will find that they are each year better able to meet the demands of the engineering profession. For character, judgment, efficiency and an understanding of men—those personal qualities that make up 75 per cent of the engineer's equipment—develop best in men who love their work and who labor with enthusiasm and intelligence at things which they know to be worth while.

DISCUSSION OF DR. MANN'S (CARNEGIE FOUNDATION) REPORT OF PROGRESS IN THE STUDY OF ENGINEERING EDUCATION.*

By James R. Withrow.

The first questionnaire sent out by Dr. Mann could only be expected to be qualitative in its results because it was unlikely that the engineers replying should think of all the topics suggested by other engineers and voted upon in the replies. These men were to *suggest* quite independently, topics of importance from the point of view being considered and also list them in the order of their importance; in other words, they were to both nominate and vote upon them. The results of the last questionnaire were more nearly quantitative because the Foundation averaged the former replies, selected the topics to be voted upon and requested a vote by the profession. It is interesting to note, however, that there is a striking tendency toward agreement, in the two votes, even from a quantitative viewpoint, as Dr. Mann has pointed out. In discovering this general quantitative agreement the Carnegie Foundation, through Dr. Mann, has made definite progress during the past year in crystallizing into a rather definite ideal the opinion of the members of the engineering profession with reference to what qualities make for engineering success or are of greatest importance in sizing up young men for employment or promotion, and it is to be hoped that the report will be distributed broadcast among engineers.

Such a report will assist teachers, while laying broad, scientific and technical foundations, to keep before them the fact that they should be constantly developing and keeping in mind the paramount importance of:

(1) Character
(2) Judgment and resourcefulness
(3) Initiative and efficiency
(4) Understanding of men

Dr. Mann's suggestions on the latter subject, human engineering, are very timely and should receive increased attention from

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teachers and the profession. The recent Congress on Human Engineering at the Ohio State University should be followed at other institutions by similar organized efforts at mutual understanding along this line.

In the list of qualities necessary for success, etc., I believe resourcefulness is a result of experience, and therefore, I should prefer to place it in the group with judgment rather than with character and integrity. Initiative is closely related to efficiency and I would head *this* group, therefore, the "Initiative and Efficiency Group," rather than place initiative in the character-integrity group.

We should not fail to ponder on the importance of Dr. Mann's statement that growth in the four qualities mentioned above cannot profitably be arrested for four years *even to obtain a more thorough technical training* and that much more efficient is the teaching of technology which gives opportunity to develop integrity, initiative, resourcefulness and common sense as well as technique.

It is true that one has little opportunity of displaying those vital qualities in his profession until he *has* become an engineer, that is, has displayed technical ability. However, *we* must not put technical training first, but develop our work, or even slow up if necessary in our technical teaching, to insure the student a growth in the character group of qualities. We cannot afford to navigate without possessing *all* of these qualities which were voted upon. He is obviously a poorly developed and "one sided" engineer who lacks even the quality with the lowest vote value. Our technical training therefore must be *based on* or involve efficient training in this character-judgment-efficiency-humanity direction (not "humanities").

I imagine most, if not all, of our science and engineering subjects are admirably fitted for such treatment, and may readily become the vehicle for developing these desired qualities so necessary for professional as well as personal success. It becomes strongly evident that the dominant feature, in discussing ideals and successful engineering training, therefore, is the teacher himself as a man. The teacher's point of view and attitude toward the utilization of his subject for consciously developing these qualities becomes of prime importance. In addition it must not be forgotten in discussing the "what" and "how" of teaching that a good teacher often obtains results by methods that are useless to even other good

teachers and I believe these ideals now laid down will act as a stimulus and a help to all teachers, be their method whatever it may.

Having apparently ascertained that the real problem of the schools is to meet the ideal laid down in this report, the report becomes a lamp in whose light the schools of engineering may judge their own work and map out their progress and keep clearly in view the goal toward which they must ever work. The question then arises—how may this goal be most closely approached and how must our fundamental technical teaching be arranged and conducted? These are the questions in which we were all originally interested but we now have a tool for measurement.

In this connection I believe that as teachers, we can profit by the profession's discussion of the "how" and "what" in teaching, before discussion by the teachers themselves, who after all must decide the matter though they should not do so arbitrarily. I hope that the Joint Committee will not neglect this help from the profession and I believe our courtesy will be appreciated by the profession.

While Dr. Mann, I believe, has been good enough to point out that the technical subjects in the engineering schools appear to be well handled yet there is much still to be desired in engineering education. For instance, in studying statistics collected by Dean Coddington of the College of Engineering of the Ohio State University when he was Secretary of that College, in the matter of the grades obtained by students in engineering throughout their four years, their curves appeared distressingly as though the engineering college work was an *endurance contest* and not a *development,* as I surmise education should be.

In Dr. Mann's report, the tests conducted on Freshman engineering students is really a test of the product of the high schools and most of the questions were apparently directed at the memory faculty. Some are aimed at knowledge of a particular ancient definition. For instance "A body of .... entirely surrounded by .... is called an ....." If the student had had a teacher who emphasized that all land was entirely surrounded by water and that the smaller portions detached from the mainland or continents were called islands, this pupil would have a real problem on his hands to fill in the desired spaces in the above question.

The first year student who could find his way through the paragraph beginning "A chief characteristic of science," etc., under

the head of *"English,"* and understand the appended questions, should in my opinion receive a diploma forthwith. It is likely he would be spoiled by an engineering course. It is to be earnestly hoped that no teacher of engineering will feel at liberty to make this paragraph and its questions his ideal for clearness.

The question in "physics" regarding the amount of water which "must be added to a pint of alcohol 95 per cent pure to make a solution of alcohol 40 per cent pure," cannot be answered from the data given. The statement that Professor Thorndike found that "only one-third" of his 220 engineering freshmen could solve the problem is interesting. It recalls the inattentive student who was asked the cause of the aurora and attempted to excuse himself by stating that he "did know but had forgotten." Whereupon the professor said "How unfortunate! The only man who ever knew has forgotten."

It is probably intended by the question to develop the idea that one pint of 95 per cent alcohol contains 95/100 pints of real alcohol and that each pint of 40 per cent alcohol contains 40/100 pints of real alcohol, therefore, one pint of 95/100 pure alcohol *will make* 95/100÷40/100 or 2 *and* 15/40 pints of 40 per cent alcohol. It would not follow, however, even if this were true, which is doubtful, that 2 and 15/40 pints minus 1 pint (95 per cent alcohol) = 1 and 15/40 pints of water *to be added* to get to get 40 per cent alcohol from the one pint of 95 per cent alcohol. This will not be enough water, for the mixture shrinks in volume. The resulting alcohol would be *over* 40 per cent strong. Because of ignoring the inherent volume change, the strength of the resulting alcohol will be in error 2.5 per cent by volume of 3.0 per cent by weight, a very grave error for scientific or engineering work.

The apparent object of the question to get at the true significance of "per cent" is commendable but *chemical* questions should not be made the basis for such a demonstration because they frequently, as in this case, teach the students false fundamental chemical ideas. In some cases which might have been selected the error due to the ignored volume change would have been much larger.

Such problems are too frequently used, thus not only missing the opportunity of calling attention to the fact that because of the volume change more data is necessary than would be expected, but still worse, the fact that such a problem is given at all (in the absence of discussion of the facts) operates to impress the student

that he has been informed that chemical compounds like alcohol and water can be mixed to obtain a final volume equal to the sum of their respective volumes, a very dangerous mental attitude, and one which would result disastrously for the chemical engineer. For instance if this problem had furnished the specific gravity of water and 95% alcohol and requested the specific gravity of the resulting mixture the calculated specific gravity would have been about 16 per cent away from that of 40 per cent alcohol. In order to avoid these difficulties the problem should have stated the weight of a pint of water and also of the respective strengths of alcohol or their specific gravities.

Serious consideration should be given to the suggestions made by Dr. Mann in regard to increasing student efficiency, and it is to be hoped that Dr. Mann will be able to suggest some particular way for carrying out these ideas. It is one of the greatest problems of teaching to cause the student who has the tendency "to soldier" to take an earnest hold of his work. It also is a problem to prevent the student who ranks high in his class from acquiring the proverbial "big head" which makes him so unsatisfactory to his fellow students and subsequent employers. Although such a student usually gets along in spite of this failing, he nevertheless has a discouraging influence on the desire of other students to stand well in their work. It is a little difficult to see how the ideas which Mr. Taylor has been able to work out in the industries can be applied to the radically different subject of teaching. Mr. Taylor's ideas were not applied as such to the sole part of industrial work which involves the teaching of methods or operations. It is probably not intended by Dr. Mann that scientific care in the setting of problems before students is not exercised at the present time in the colleges, for most teachers at least of technical subjects undoubtedly give this matter serious and careful attention and many of them have very gratifying results. We must not forget also that the large portion of the student's work is of the nature of drill. This being the case it is hard to see how in all classes the tasks of the students can be arranged from easy to difficult though naturally every teacher arranges his work, as far as possible, in this way, otherwise the student takes little interest in his work.

It will be noticed from Dr. Mann's discussion that in the industries great pains are taken to see that each workman is carefully instructed in the most efficient methods of doing his work. I

do not believe that as a rule in the colleges much effort is put forth by our institutions in teaching our students how to study and yet most all teachers agree that they do not know how. It seems as though some method should be arranged whereby we should have more extensive application of the idea which I understand is used at some institutions of taking the entering student and giving him explicit instructions in the proper methods of studying the work which he is to undertake. This matter, I think, cannot be emphasized too strongly by Dr. Mann. There are great difficulties in the way of doing this thing, but any effort made would surely be worth all that it would cost.

Some of the illustrations as given by Dr. Mann of the interest aroused in certain work are not very clear. For instance, when it is stated that freshmen in chemistry are given problems in one school very similar to those given in other schools to *seniors* in chemical engineering it is hard to understand just what is meant. It is, of course, a fact that in giving work to advanced students we are compelled incidentally to use means which even freshmen can undoubtedly handle. They can all spell and they can all count or figure, they can all use the fundamentals of the science when they are seniors, at least they should be able to, but it would be unfortunate if the impression got about that because the freshmen can also do this that they are doing the work of seniors in chemical engineering. It is obviously impossible for any freshman to do the work which is the real work being carried out by seniors in any real chemical engineering course. In the first place they have not yet had the chemistry and in the next place they have not yet had the engineering, and so obviously such statements are subject to misconstruction though they doubtless have relation to some fundamental idea. It is perfectly true, for instance, that a freshman in chemistry may be asked in making anything to figure out how much it cost *him* to make it so that he may be doing some work which a senior also may do, but while manufacturing cost calculation is properly part of the work of advanced students in chemical engineering it is only a very small portion of their work though an important portion. In any event it is a matter which no freshman can adequately elaborate for the reason that he has not had enough of the fundamental or advanced work in either chemistry or engineering. He can handle the matter only in a general fashion and the result will be, that if an effort is made to go into such matters in

anything but the most superficial manner valuable fundamental material must be left out for lack of time, and when the man reaches his advanced work the instructor here will be handicapped by the constant necessity for teaching fundamental work which should have been taken in the first year and the student may get false ideas of the commercial value of his results. However, I believe in utilizing anything in the teaching of fundamentals that can bring better results, always, however, calling attention carefully to the fact that exhaustive or technical treatment is not intended. I agree very thoroughly with Professor Mann in his objection to the presentation of any work in an abstract manner. The presentation of mathematical or chemical puzzles to students when concrete cases can be given is not the way to teach any science, in my opinion.

Since the New York Meeting of the Institute the Society for the Promotion of Engineering Education forwarded the discussion of Dr. Mann's Report which had taken place at the University of Virginia and gave opportunity for comment by your chairman, of which the following is a copy.

Professor Sperr in the discussion has called attention to a matter with which I am in hearty accord when he emphasizes to what extent it is the teacher's business to reduce as far as possible the percentage of failures in his classes. It is too often a matter of pride among teachers that their course is so stiff that only select material can succeed in passing it or that it is reputed to be a difficult course. It should rather be the teacher's pride that he has been able to help students rather than rule them out. Any method, such as Professor Sperr seems to have pursued, which the teacher can devise to this end, makes him an increased benefit to the community which it is his duty to serve, as well as to the individuals who come under his care and whose future is to a considerable extent in his hands.

Dean Cory in the discussion has called attention to the method used at his institution for saving students who showed backwardness from any cause, by interviewing them at the end of six weeks' periods. This method I can heartily commend because it sounds similar to the one that was applied to myself as a student at the University of Pennsylvania twenty-two years ago, with very satisfactory results from the student's point of view. Even tendencies which in time would result fatally to the student can be detected and eliminated by such personal touch. Dean Cory has also called

attention to the necessity from time to time of checking up the character of the work itself which is being given to the students. This is frequently of assistance to the teacher as he is apt in the strain under which he works to acquire a biased or improper attitude towards even his own work, and it does not pay him to be unfair to the student, and the sooner he learns of it the better for all concerned.

Dr. Mann in the discussion suggests that the fact that technical subjects appear more efficiently taught than the theoretical ones, is probably not because the technical men are better instructors or better masters of pedagogy, but because they are in more immediate touch with real things and the student gets a conception of the fact that the problem is real. Without question this situation helps much in the teaching of technical subjects. We must not overlook the fact, however, that these teachers of technical subjects have an additional advantage over most of the teachers of theoretical ones, an advantage, however, which these teachers could largely overcome if they cared to. This advantage consists in the fact that the man who is used to the responsibility of commercial problems which must be solved and solved at once, in other words, who is confronted with the proposition of being compelled *to get results,* will naturally have a different attitude towards his teaching of any subject and will go at it in a different way and more from the point of view of vital fundamentals, than a teacher of any other experience can do, though these other teachers may acquire ability to a varying extent in this same line if they choose. So that the point of view and mental attitude of teachers in technical subjects is usually quite a different one from that of other teachers, even though they happen to be excellent teachers of subjects where these teachers are not themselves forced to handle their problems in a businesslike way. This is an advantage in attitude which all teachers should aim to possess if they can secure it and I believe it can be secured if the will exists. Mr. Dooley in the discussion has called attention to the success he has had in his Technical Night School where the teachers are not trained ones and who do not know the theory of teaching. As he says, by their intimate knowledge of the actual work and their interest in helping young men along, they are well qualified for the job. It should be noted in addition that they are men who are used to commercial responsibility, to being called upon to meet situations and to bring results,

and at once, if possible. This, in my opinion, will make any informed man of breadth of view a good teacher if he is interested in teaching. If we are engaged in research it helps our teaching ability, but teachers may be engaged in research which is merely an interesting problem which can be taken up and laid down at pleasure and which never needs to be completed except we have the inclination to do so. In other words, we are never held to an account. No pressure except our own good pleasure is brought to bear upon us for results. Such work is certainly much better than no research work at all, but it lacks the training to the teacher himself of work which involves responsibility and must be done in a specified time or under pressure.

I am not competent to discuss the experiment on mechanics in the first year which Dr. Mann has mentioned, but it sounds very interesting and I hope will be successful. We must beware, however, of drawing general conclusions from educational experiments which may be very attractive or successful because of the personal enthusiasm of their inventor. This is one thing which makes education so complex. Frequently a method which is very successful in the hands of an enthusiast for that particular method may bring little results from the hands of some one else who is not so enthusiastic regarding the method but who may be just as enthusiastic in the science or subject. In other words, we must not be misled by isolated experiments.

With regard to Dr. Mann's question, "How shall we make the engineer more cultured?" that is a matter upon which I would like to see discussion, first, from the point of view of "What is culture?" and second, "In what way our science and technical subjects lack it." My understanding of culture is one thing, and perhaps others have another idea. I do not believe myself that any man leaves any university cultured, though he may be given a point of view which may enable him to acquire that state. I believe we can do much in the schools to develop the human side of the man and I am greatly surprised if in the technical sides of education, at least, much is not now being done in this direction, as well as giving the student a mastery over matter. I have the idea that technical subjects can be intensely human.

The CHAIRMAN: You have heard the report of the Committee on Chemical Education. Is there any discussion? If not, the report will be received.

THE METALLURGY OF THE RARER METALS

By J. W. RICHARDS

Read at the Cleveland Meeting, June 14, 1916

There are many metals which may be called "the rarer metals." Amongst them the most interesting by far, to the metallurgist and to the economist, are those metals whose compounds are relatively cheap but which command a high price because of the difficulty of their reduction. These are the metals whose market price may at some time be reduced one-half, three-quarters, perhaps nine-tenths, by improved methods of reduction, and the discussion of how this might be accomplished and to what uses these metals at such low prices might be put, is interesting to the border of fascination.

If this article were being written thirty years ago, aluminium would be one of the metals to be discussed. It is now out of that class, but in 1886 it was one of the rarer metals, selling at $10 per pound, although its ores were "as common as dirt." At that time you could buy a ton of bauxite ore containing 6/10 ton of alumina or 3/10 ton of aluminium, for $5, while the 3/10 ton of aluminium was being sold for $3000 at wholesale. Or, eliminating the purely chemical work, 6/10 ton of chemically purified alumina could then have been bought for $50, while the aluminium it contained was worth on the market 60 times that amount. Such were the metallurgical conditions in the aluminium industry thirty years ago, and so attractive were they to experimenters and inventors that the genius of the profession expended its talents on the problem, with the result, in less than ten years, of reducing the market price to less than one-tenth its former figure.

The silicon industry furnishes another example in point. Silica is the most abundant and cheapest material in nature, yet silicon was selling in 1900 as a chemical curiosity at *over $100 an ounce*. Imagine the stirring up which my metallurgical wits received, when in 1902, Mr. Tone, at Niagara Falls, showed me a barrel full, a hundred pounds perhaps, of silicon made in his electric furnace,

and asked me what uses it could be put to. At the present time, ten cents per pound is a good market price for silicon, which is often sold by the car-load.

It is such seeming fairy tales as these which constitute the fascination of those metals which are abundant in nature but whose high cost rests on the difficult and costly methods of reduction employed. Such opportunities exist for our rising generation of chemists and metallurgists to make themselves famous and, incidentally, rich—but they will find their fame more of a reward than their riches.

Among the metals at present of high price, but which by improved metallurgical processes might be made very cheaply, are beryllium, boron, magnesium, calcium, strontium, zirconium, molybdenum, barium, titanium, chromium and cerium (mixed metals of the cerium group).

BERYLLIUM

Commencing with one of the light alkaline-earth metals, its ore is not as rare as is ordinarily supposed. Most non-mineralogists link it with beryl, and think of the latter as a very pretty and very expensive gem—the emerald. But the emerald is only the clear green or aquamarine stone, while massive beryl, looking like massive green quartz, is much more common, and is even abundant in some localities. The beryl crystals of Acworth, N. H., are sometimes as large as a barrel, and the massive beryl at this locality is quarried like feldspar. Its composition is

SiO_2	67.0 per cent
Al_2O_3	19.0 per cent
BeO	14.0 per cent

This material can be bought for $90 per ton in car-load lots. When chemically treated, both the Al_2O_3 and the BeO which it contains could be separately obtained.

Up to the present, no one has succeeded in isolating the metal except by reducing a halide salt of beryllium by potassium or sodium (Bussy, Wöhler, Debray, Menier, Reynolds, Nilson and Petterson, Kruss and Moraht), or by electroylsis of double chloride, bromide, or fluoride of beryllium and sodium or ammonium (Borchers, Warren, Lebeau, Liebmann).

These methods are tedious and costly for two reasons: first, they require the conversion of the beryllium into an anhydrous halide salt, which is a difficult chemical operation, and second, the electrolysis sets free the halogen, which is very destructive of electrodes and apparatus. It is not to be expected that beryllium can be made cheaply until some one masters the direct electrolysis of the oxide, dissolved or suspended in a more stable melted salt. This is by no means an impossibility; a similar solution was found for the aluminium problem, and systematic, determined search would in all probability find the answer for beryllium. In such a case, the cost of the metal would depend only on the cost of beryllium oxide, being probably not more than 20 cents per pound plus the cost of the oxide. Since BeO is only 36 per cent Be, it would require 3 pounds of oxide to give one of metal; if the oxide cost 25 cents per pound, the total cost of the metal should not exceed 90 cents, by such a supposition process. Dealers in rare chemicals will charge you, at present, $300 per pound for a specimen of it.

Lebeau has produced beryllium bronzes by reducing directly, in the electric furnace, a mixture of beryllium oxide, copper oxide and carbon. 0.5 per cent of beryllium makes copper hard and sonorous, 1.5 per cent makes it yellow, and 5 per cent makes a fine golden yellow bronze.

Beryllium, like any other rare metal must find uses which justify its cost. Being white, malleable and unchanged in air, its specific gravity, 1.64, would make it particularly useful for objects where great lightness and permanence in air is the first consideration and cost secondary. So far we practically know nothing about its tensile strength or rigidity, about how it might be strengthened or stiffened by small additions of magnesium or aluminium or even of zinc, or copper, or manganese, or some other metal. We do not yet know the mechanical properties of its fine bronzes, except that they are somewhat similar to aluminium bronze, but in what respects they might be superior or perhaps unique, is unknown. Finally the metal may easily possess special properties, now unknown, which may render it particularly useful for some specific purpose. Its specific heat, for instance, is the highest of any useful metal, and its latent heat of fusion must be abnormally high, possibly 300 calories, and its latent heat of vaporization probably higher than that of any known element except carbon or boron. Such characteristics might give it special uses in electrical instruments, or

for physical apparatus, where its cost would not exclude its use. Altogether, beryllium is a metal which will well repay extended metallurgical research and minute physical and chemical study of its many unique properties.

MAGNESIUM

The metallurgist has been coquetting with magnesium for half a century, and has, as yet, not made a fraction of the progress which he should have made. We can get its oxide cheaply and in abundance, its salts are not very difficult to prepare, we know their properties to a considerable extent, we know almost all the properties of the metal which bear on its isolation, and yet the industry lags and halts as if there were no such thing as modern metallurgy. To prepare by tedious methods the anhydrous double chloride, and then to electrolyze it about as Matthiesen did fifty years ago, is nearly all that can be said with certainty about its present metallurgy. At any rate, using magnesium oxide costing a few cents per pound, the metal sells for about as many dollars per pound, and yet there is a great scarcity of the metal.

Dr. W. M. Grosvenor, in a recent paper before the American Electro-chemical Society, summarizes the present methods of production and the uses of the metal. Happily he also suggests the great field open for improvement, especially for radically new metallurgical methods of production. With magnesium in its salts costing less than 8 cents per pound, he places the actual cost of the metal at $1 per pound, leaving over 90 cents per pound for the cost of its extraction. "Brethren, these things ought not so to be." Speaking with the enthusiasm born of past achievements in electrometallurgy, along entirely analogous lines, a modern, up-to-date attack on this problem ought to result in producing magnesium at 25 cents per pound.

The point of attack should be undoubtedly to reduce the oxide directly. The halogen salts are hygroscopic, and the halogen is destructive of the reducing apparatus. It is almost certain that proper research will enable the electrometallurgist to feed MgO directly into an electrolytic bath of fused salts, and take magnesium or magnesium alloy from it. The pure metal will float on almost any fused salt, but its alloy with heavier metals may be made such as to sink, and many of its alloys have immediate useful applications.

Dr. Grosvenor makes suggestive remarks about the reduction of magnesia by carbon. If the boiling point is only 1200°C, then a process of reduction similar to that of zinc oxide might be practicable if we could (1) find a retort material which will stand the temperature required (1800° to 2000°C), and (2) condense the vapors without contact with air. Fortunately, magnesium does not form carbide at high temperatures, so that its reduction is simpler by that much. Dr. Grosvenor speaks of a chemical process which will use cheap raw material, a moderate amount of fuel, and give a fair efficiency of reduction. With all expenses added, he estimates a cost not over 35 cents per pound. This may be true, but whether it materializes or not, he and his colleagues have the right vision of the possibilities, I may even call them the probabilities, of this field; they are truly "absolutely fascinating."

While the world war lasts, with its enormous demand for magnesium for military purposes, the price will remain in the dollars per pound. But experience in this line is being rapidly accumulated, and improvements are undoubtedly rapidly succeeding each other, although keen competition is keeping them secret as far as possible. After the war's close, with normal industrial conditions reappearing, magnesium will undoubtedly sell at a price which will take it out of the class of the rarer metals and put it among the common ones. As the price goes down its industrial uses will increase in a geometric proportion, and instead of production being expressed in thousands of pounds per year it will reach thousands of tons. This will be another of the by-products of the great war's stimulus to metallurgical industry.

The possibilities held out to the metal industry by reasonably cheap magnesium are extremely interesting. The stiffening of magnesium to produce strong alloys with specific gravity not over 2, has not been properly studied. It is quite possible that alloys analogous to *Dur-alumin* may be discovered, as strong as soft steel and only thirty per cent of its weight, which will find extensive use in aeroplanes and dirigibles. Such alloys may also largely displace aluminium alloys, which are used by thousands of tons annually in the automobile industry, with a saving of one-third in weight, which will compensate for a higher first cost. The metallurgical uses of magnesium will also be greatly extended by its lower price, such as for deoxidizing brass, bronze, nickel, monel metal, since it is a much stronger deoxidizer than aluminium. In fact, alumin-

ium has blazed the way into numerous uses for which magnesium, as soon as it becomes cheaper, will compete and replace its older sister. With supplies of magnesium ore as plentiful as those of aluminium ore, and the metallurgist awake to his responsibilities and producing the metal cheaply, there will inevitably be a large future for magnesium as one of the common metals of every-day life.

CALCIUM, STRONTIUM, BARIUM

These are a trio of highly interesting elements, common enough in nature, but all scarce and of high price because of the metallurgists' lack of efficient and cheap methods of reduction. With burnt lime, CaO, one of the cheapest of common materials, strontium sulphate, a mineral found in considerable abundance, and barium sulphate so common as heavy-spar that it is used as an adulterant for some cheap paints, the metallurgist is again faced with the demand for cheap methods of reduction. And yet, although calcium is sold at a few dollars a pound, strontium and barium cost several dollars per ounce. Here is a twofold need: first, cheap production; second, a thorough study of these metals to find out their specific properties and their particular uses. Which should be undertaken first is an interesting topic for discussion. Historically, the metallurgist has usually produced the metal first and then studied its properties and possibilities; at present, with these metals already at hand, metallurgical activity might be greatly stimulated by extensive studies of the properties, alloys, and chemical uses of these elements. Our present information in this direction is fragmentary and partly unreliable as far as it goes. A Carnegie Research Scholar, or even the Bureau of Standards, by disclosing to us some of the unknown properties of these elements, might stimulate the experimenter to renewed efforts to find cheaper methods of reduction.

Calcium, at the present time, is the best known of these three elements. The method of electrolyzing its fused chloride and lifting the metal away from the surface, as an irregular stick, has been fairly successful, and since the chloride is not difficult to dehydrate, the whole operation is not very expensive. Calcium is therefore, at present, perhaps a semi-rare metal, which could be produced much cheaper even by present methods if made on a large scale to fill a large demand. The method of production is easily

susceptible of minor improvements, and the chlorine is a valuable by-product to the manufacture. The principal hitch at present is in finding the uses for a large production of calcium. Here is where extensive study of the properties and possible uses of calcium would greatly stimulate the metallurgical industry. With specific gravity of 1.85, its possible alloys with other light metals should be exhaustively studied; quite possibly some of them are strong, resistant to air and water, perhaps even to acids. Calcium tarnishes easily in the air, and magnesium also, but it is quite possible that some alloy of the two does not tarnish, and may have valuable mechanical properties. Another large possible use is as chemical purifying agent in melting and casting metals. Calcium-silicon-aluminium alloy has already found application as a deoxidizing agent in steel, because while aluminium oxide and silicon oxide and their combination with each other are infusible at steel-melting temperature and therefore are eliminated slowly from the metal, calcium oxide forms with these an easily fusible slag, which easily rises out of the molten metal. It is quite possible that a small addition of metallic calcium may in a similar manner reduce the amount of sulphur and phosphorus in steel, because it is either as calcium sulphide or calcium phosphate that these elements are eliminated in refining steel. Other metals and alloys whose properties are damaged by sulphur or phosphorus, may be similarly refined or improved. The alloys of calcium with copper, tin, bronze, brass, monel metal, and other commercial alloys have not been studied; until they are, no one knows how many useful mixtures may exist with particular properties of industrial value. The question of adding calcium to the light stiff aluminium alloys, for instance, is worthy of attention, but has not been touched.

Strontium is a silvery white, very soft metal, with properties similar to calcium, density 2.54. Its ores cannot be called rare minerals, and it is a rare metal, therefore, only because of the difficulty of its isolation.

It is chemically very active, and electrochemically extremely hard to manage. It has about the same specific gravity as its fused salts, so that it neither rises nor sinks in any of them quickly; it seems also to re-dissolve in its melted salts with great velocity, so that very high current density is required to obtain any metal at all. Its surface tension appears to be abnormally high, so that it separates out in more or less minute globules which are highly in-

disposed to running together into one mass. What an attractive subject this forms, to the electrochemist who really wants to meet difficulties and taste the joys of overcoming them. And then, one may well ask: To what purpose? Here, again, we do not know, but we can feel confident that in the innumerable list of possible combinations of metals strontium might have properties different from any other element, which would lead to its employment on a large scale. These are all questions of the future which form the undiscovered country open to the investigator and chemical pioneer. We may well thank our stars that the world of science still holds unexplored areas to tempt the adventurous investigator and to reward the *wanderlust* of the metallurgical pioneer.

Barium is common in its compounds and almost unknown in itself. Under electrolytic conditions where calcium comes out *en masse,* and strontium separates as small globules, barium is obtained only as a fine powder. Its density is 3.75, but its salts are heavier than the corresponding strontium and calcium salts, so that the fine powder may sink or swim, largely according to the temperature. And yet the fused barium salts are very decent and manageable salts, easily obtained, anhydrous and melting cleanly. Their electrolysis, however, is nearly the most difficult task that the electrometallurgist can take up. But it should be taken up and mastered, because a metal so common in its compounds could be obtained in large quantities if uses for it were developed, and if our modern electrometallurgists are worthy successors of Faraday, Bunsen and Castner, they should vigorously attack the problem of its cheap extraction. Even if their end was not reached, they would be whetting their metallurgical wits upon the finest of chemical whetstones, and their experiences would be of value to themselves as well as to other lines of electrometallurgy.

BORON

As an intermediate element, semi-metal, cheap and abundant in its compounds, yet almost unknown as an element, boron is very interesting. Although quoted in dollars per ounce, yet there are possibilities of it being made for x cents per pound, where x cents may be anything over 25. It is only the prophetic prescience of the enthusiastic metallurgist, however, that can discern this goal in the distant future.

Boron occurs in nature as its oxide sassolite, containing 31 per cent of boron, as borax, containing 11.5 per cent of boron, and as colemanite, containing 18.9 per cent of the metal. These sources are comparatively abundant, the oxide being found in volcanic districts, borax near dried up lakes, and colemanite (calcium borate) being literally a waste product of the borax mining for which no use has yet developed.

Gay-Lussac and Thenard, Wöhler and Sainte-Claire Deville reduced the oxide by potassium or sodium, other chemists by phosphorus, magnesium, aluminium and calcium, while Duncan reduced boron chloride gas by hydrogen. Chemically, its reduction does not appear to be of extraordinary difficulty. Electrochemically, Davy electrolyzed fused boracic acid, Gore potassium borofluoride, and Faraday fused borax. All describe having isolated boron. Quite recently, Weintraub has decomposed boron chloride by hydrogen in a high-tension arc, and obtained purer boron and in larger quantity than any previous investigator. His classical paper is in the Transactions of the American Electrochemical Society (1909) 16, 165.

The properties of the pure boron obtained by Dr. Weintraub are exceedingly interesting. Its fusing point is extremely high, between 2000° and 2500°C; it was fused in a boron nitride crucible under the pressure of its own vapor. Its boiling point is near to its fusing point; it has considerable vapor tension as low as 1600°C. It has a conchoidal fracture, and is nearly as hard as the diamond. At room temperature it is electrically almost a non-conductor, but at 500°C its conductivity has increased 2,000,000 times; at 1000°C its conductivity is of the order of that of the metals. Applying moderate voltages to a cold piece, it soon warms up and makes itself a good conductor. Such extraordinary properties suggest its use for many interesting electrical contrivances, which are enumerated by Dr. Weintraub.

Other uses of boron, which have not yet been thoroughly investigated, are in the formation of boron steels and boronized copper. The former were investigated by Guillet, in France, with irregular results; in some respects, at times, the effects were similar to that of vanadium in the famous vanadium steels, at other times the results were different. The uncertainty may have been due to irregular composition of the ferro-boron alloy used. A great amount of investigation should be done on this line, first in making

a reliable quality of ferro-boron, and second in using it systematically in various qualities of steel. The question of boronized copper is in a still greater state of uncertainty. Boron or even boron sub-oxide (B_2O), or, perhaps, boron saturated with B_2O_3 added to melted copper enables a perfect copper casting to be obtained, of practically 100 per cent electrical conductivity; a trade product sold as boronized copper has similar effects in producing sound copper castings. These results are only the beginning of an extensive field of investigation of the effects of small amounts of boron on metals and metallic alloys. They will certainly lead to important metallurgical discoveries and improvements.

In metals and alloys boron can act chemically, as a purifying or refining agent to remove oxygen, nitrogen and perhaps sulphur, phosphorus, and dissolved oxides, while in larger amount it acts metallurgically as an alloying element. In the latter respect it forms true alloys, such as the ferro-boron alloy, which is an article of commerce. The possibilities of extensive use in molten metals and alloys are great, but mainly dependent upon systematic metallurgical research in properly equipped laboratories.

The production of the metal and its alloys also needs expert attention. For use in steel, a good uniform quality of ferro-boron is needed, and the manufacture of this in satisfactory uniform quality has not yet been mastered. Some years ago, the Pacific Coast Borax Company offered a prize of $500, to be awarded by the American Electrochemical Society, for a practical electric furnace method of producing ferro-boron directly from calcium borate (Colemanite), a waste product of the borax mines. A few years later the prize money was returned by the Society to the Company, as not having been earned, although several attempts were made at it. For use in copper, brass and bronze, cupro-boron alloy answers as well as having pure boron. One method of making this is being tried, and the product is giving some very satisfactory results. Improved and more certain means of getting boron into copper are needed, and could probably be found by a moderate amount of careful investigation. As for pure boron and the fascinating possibilities dependent on its remarkable properties, Dr. Weintraub's method makes the product, but at considerable expense, and a cheaper, easier method is a great desiratum. If such is found, boron will certainly occupy an important place among the useful metals.

CHROMIUM

This element is also common and abundant in nature, and rare and expensive as a metal. Chromite, containing 34 per cent of chromium, costs normally $20 to $25 per ton, while the ferro-chromium alloy produced from it sells at $100 to $550 per ton, according to the per cent of chromium and carbon contained. But pure chromium, carbon free, is produced only by reduction of chromium oxide by aluminium, and commands 75 cents per pound. The use of chromium in steel is rapidly extending to all varieties of extra hard and high-speed steel, but the use of pure chromium is limited by the high cost of its production and our lack of knowledge of how to handle it and of its possible useful effects. For example, chromium electroplating is white and durable, and for many purposes may be superior to nickel and almost equal to platinum plating, but the technique of always getting perfect plating has not been satisfactorily mastered. Cobalt-chromium alloys have been made which have some of the remarkable properties and uses of high-speed tool steel (Stellite alloy of Mr. Haines). This is an excellent example of totally unexpected and valuable physical properties being discovered by systematic investigation. These alloys, however, must be made from pure chromium, and not from ferro-alloy. How many other remarkable alloys yet remain to be discovered, by patient and intelligent investigation, no one knows or can even guess.

As for the methods of reduction, ferro-chromium alloy carrying high carbon (6-8 per cent) is produced quite cheaply in crucibles, cupola furnaces, blast furnaces, or electric furnaces. Low-carbon ferro-chromium commands three to five times as high a price, because of the difficulty of decarbonizing the raw product. It is very much to be hoped that tests will be made in the electric shaft-furnace to see if it is not possible to produce directly from the ore a low-carbon product. The thing has been done in the case of pig-iron, producing a low-carbon product which is called pig steel; there is no inherent impossibility in similarly mastering the conditions for producing directly the low-carbon ferro-chromium. The present prices of the two products, $100 and $500 per ton respectively, would warrant great efforts in that direction.

Similarly, chromium is not a difficult metal to reduce to the metallic state, but it is a difficult question to find the proper flux.

and to keep carbon out of it. Goldschmidt reduces it by aluminium; electrolysis of its fused salts is difficult because of their high melting points. If electrolysis of aqueous solutions of chromium salts could be satisfactorily controlled, so as to produce heavy deposits, this might open the door at once to cheap and pure chromium. Electrolysis of molten relatively fixed salts in which chromium oxides are dissolved (similar to the Hall aluminium bath) is not a hopeless proposition. The chromium would be plated solid, however, on chromium cathodes, since the working temperature would be below the melting point of chromium.

The metallurgy of chromium is full of attractive possibilities, and the usefulness of pure chromium in the field of alloys is only beginning to be scratched; the scratching, however, is proving very much "worth while."

TITANIUM

Our friend, Mr. A. J. Rossi and the Titanium Alloy Manufacturing Company of Niagara Falls, are the *alpha* and *omega* of the titanium industry. Nearly 15 years ago my first introduction to Mr. Rossi was in the historic barn, at Niagara, the cradle of so many of the Niagara Falls industries. Mr. Rossi was running the electric furnace and Mrs. Rossi was in the little laboratory, making the necessary analyses.

Everyone knows of the enormous masses of titanic iron ore in northern New York and Canada, which contains so much iron and so little sulphur and phosphorus, that every blast furnace would be glad to get it if it was not for the titanic oxide which makes it so unworkable that you could not give it away to them. Mr. Rossi tried first to extract the iron only, throwing away the titanium in the slag, but that was not profitable. He then turned to producing ferro-titanium alloy, for use in steel and cast iron, and by indomitable perseverance has made that a success. A finely written booklet of over 100 pages, published by his company and to be had for the asking, tells the whole story, so why take up time to rehearse it here? Get the booklet and read it.

In addition to titanium treatment of steel, to deoxidize and denitrogenize, this company also makes a specialty of titanium-treated aluminium bronze, also of titanium-treated bronzes and brasses of various compositions.

With titanic iron ore carrying 10 to 15 per cent of titanium as

cheap as iron ore, and if iron-free material is required, rutile, 60 per cent titanium, at 5 to 7 cents per pound, there is no lack of cheap raw material. If uses are found for pure titanium, however, some other than the electric furnace must be used to reduce it, because, in absence of iron, titanium carbide would result. The methods for producing pure titanium are, like its prospective uses, still in the future, but they nevertheless are worth study and work. One can buy titanium metal now at about the price of silver, but if the problem were properly faced it could probably be made as cheaply as chromium.

MOLYBDENUM

Molybdenum sulphide, MoS_2, 60 per cent molybdenum, looks almost exactly like graphite, and is about as widely distributed. Normally, it can be purchased as 90 per cent concentrates at 25 cents per pound. This would make the raw material for one pound of molybdenum cost nearly 50 cents; the selling price of the metal is about $2. This leaves a large margin to pay for reduction. In fact, $1 per pound of contained molybdenum ought to return the reduction works a good profit.

However, the principal need of the molybdenum industry is a better utilization of its sources of raw material. The deposits have not been in general properly prospected or opened up, and then not properly worked. They are usually low-grade propositions, with 5 to 10 per cent of molybdenite disseminated through hard rock. This calls for careful study of crushing and concentrating methods, so as to minimize waste and loss. In most cases, the actual treatment falls far short of this, and possibly half the molybdenite in the ore is lost. The producers of the concentrates are being paid high prices for their material, but the market is limited, and dull. If molybdenite were sold cheaper, there is little doubt that ferro-molybdenum, 50 to 85 per cent molybdenum, could be sold at half its present price, and the uses of molybdenum in steel correspondingly increased. New uses have also been found, such as the molybdenum wire so useful in electric resistance furnaces. This wire is scientifically very useful in that it resists the alloying action of many liquid metals even at very high temperatures. Dr. C. G. Fink of the Edison Lamp Works has studied these notable physical and chemical properties, and has described them in the Transactions of the American Electrochemical Society (Vol. XVII, p. 229, 1910)

ZIRCONIUM

We may mention this element, which, as metal, is rare enough, but whose oxide has recently been found in considerable abundance. The familiar mineral Zircon is the silicate, containing 50 per cent of zirconium, but it is found only in limited amount. In the last ten years the oxide, Baddeleyite, has been found in large quantities in Minas Geraes, Brazil, running 75 to 95 per cent pure, giving 50 to 75 per cent of the metal. This source is now so common that it sells at 4 to 5 cents per pound, and is being used in large quantities as a refractory material, on account of its high melting point (2000°C), high resistance to all kinds of slags, low thermal conductivity and low coefficient of expansion.

The metal, however, is almost an unknown quantity. It has been obtained by the action of potassium or sodium on the anhydrous fluorides. Fused, it is white, density 6.4, melting point 1500°C, hard enough to scratch quartz. Ferro-zirconium has been made in the electric furnace, and used in small amounts in steel, with rather indefinite results. And yet, if some very useful properties of zirconium were discovered, the metal could undoubtedly be prepared at a reasonable price—only a fraction of the $5.00 per ounce now asked for it as a chemical curiosity. How it is to be obtained cheaply is one more of the interesting questions confronting the metallurgical pioneer.

CERIUM

There is a peculiar interest attaching to this metal and its close associates, from the fact that hundreds of tons of fairly rich cerium material is lying on the waste heaps outside of the incandescent mantle factories. The thorium ore used by these factories is Monazite—a phosphate of the cerium earths plus thorium silicate. On extracting the thoria, the residue is worthless for mantle fabrication. It is known as "Commercial Cerium Carbonate," and contains cerium, lanthanum, neodymium, praseodymium, samarium, gadolinium, yttrium, ytterbium, a little thorium, and considerable alkalies, iron, phosphoric acid and silica. By treatment with acids, precipitation of the rare earths, and ignition, a chocolate-brown mixture of oxides of the rare earths is obtained, in which cerium oxide predominates, and which contains nearly

50 per cent of metallic cerium. When this mixture is reduced directly, without further separation, to the metallic state, an alloy of the rare earth metals is obtained which is known as mixed metal (mischmetal) or impure or commercial cerium. The composition of mischmetal naturally varies, but may be taken as approximately 30 to 50 per cent of cerium, 15 to 25 per cent of lanthanum, 10 to 15 per cent of the didymiums, up to 20 per cent of the yttrium metals and 1 to 5 per cent of thorium.

An immense amount of laboratory and practical work has been expended on the production of mixed metal and of purer cerium. Professor Muthmann and his students in Munich and Alcan Hirsch in the United States deserve particular mention for their articles in Liebig's Annalen (1902 to 1910) and Trans. American Electrochemical Society (1911), 20, pp. 1–102, respectively. Dr. Auer von Welsbach was the pioneer in the commercial manufacture and use of mischmetal. Annoyed by the sight of heaps of the cerium residues around his mantle factories, he experimented with their reduction to mischmetal and with the possible uses of the latter. Finding that it gave off sparks when scratched he conceived the idea of using it in automatic lighters, but found that it sparked far too feebly and unreliably to be practical. He then thought that if he purified the cerium that it might give sparks more freely, but on making the purest cerium he found it to spark less than the impure metal. Turning in the opposite direction, he took mischmetal and added to it alloying metals not of the rare earth class, and found that they increased the sparking property. Iron, for instance, when increased to 30 per cent gave an alloy with remarkable spark-giving properties, such as make it most efficient and reliable in automatic lighters. This has formed the basis of the "pyrophoric alloy" industry, since, although other metals have similar effects, the 30 per cent iron alloy is probably the best sparking alloy, for general use, so far made.

The electrolysis of fused cerium salts, double chlorides or double fluorides, to give the melted mixed metal is carried on on a large scale in Austria at Treibach, in Germany near Berlin, and has been commenced in the United States, in 1916, near New York City, by Mr. Hirsch and his associates. The technique is not easy to master, and all the works keep their operations as secret as possible. The principal difficulties are the re-solution of deposited metal, metal fog, and scattering of the metal as fine globules

or "metal-mush" through the electrolyte, making it difficult to unite the metal to one melted mass. Since the latter trouble is largely due to surface tension, a study of this property, particularly how it can be diminished, might help in overcoming the difficulty. As an example of the opposite effect, the cupelling of lead on a bone-ash muffle depends absolutely, for its success, on the surface tension of the molten lead. But, metallic tellurium in quite small percentage, *decreases* the surface tension of the lead so greatly that the metal *wets* the cupel and cupellation is rendered impossible. Arsenic, on the other hand, *increases* the surface tension of melted lead, and is therefore purposely added to it, about 0.25 per cent of it, to lead being made into lead shot, in order to make rounder shot. A physical study of such effects on cerium might well assist in overcoming the scattering of the metal in globules in the electrolyte. Another direction in which improvement might be made would be the careful study of the eutectics of mixtures of cerium salts with barium salts, so as to find an electrolyte of lower melting point in which the losses by re-solution of deposited metal would be less than they are at present. Another possible improvement would be the finding of an electrolyte which would dissolve cerium oxide or the mixed oxides directly, and give metal by electrolysis. Such a bath has been discovered for aluminium oxide, and a long experimental search for a similar bath for the cerium oxides would be amply justified.

The possibility of using the 30 per cent iron alloy as a melted cathode, and enriching it in cerium by electrolysis, might be considered. If we had the fusing point curve of cerium-iron alloys we could draw some useful conclusions in this direction. The addition of small amounts of other metals to the bath, so as to produce other useful cerium alloys directly, might facilitate the electrolytic operation. Further experience with the process, particularly by those familiar with electrolysis of molten baths for sodium, magnesium or aluminium, will very probably lead to considerable improvements and reductions of cost.

The uses of cerium and particularly of its alloys is sure to increase, and may attain considerable proportions. It has been proposed by Borchers as an addition in small quantity to aluminium, to improve its properties. But the large use will always be the pyrophoric alloys, which have so largely replaced matches. Be-

fore the European war, over 3000 workers were employed in Austria in this industry of pyrophoric alloys and automatic lighters. Mistakes were made in the early days of the industry, and some alloys put into lighters which crumbled to pieces by the time the apparatus reached Australia, but continual improvement was being made, until a satisfactory substitute for matches was attained. The improvement most needed in the pocket lighting apparatus is to be able to dispense with alcohol or similar liquid; a wick impregnated with a *solid* combustible which can be ignited by a pyrophoric alloy, would give a great impetus to this art.

The author apologizes for making this paper so largely critical and prophetic. If it has sounded too prospective in tone, the excuse must be that it is inherent in the subject. Some of these elements are among the most common in nature, yet they remain so rare in practical life that they are still chemical curiosities. But the chemical curiosities of one age have frequently become the chemical commonplaces of the next age, and even within a generation some of us have seen this happen, under our own eyes. Let us then be fore-sighted, fore-handed, and anticipate possible developments a little by such a general review as has just been given, so that when some wonderful changes and improvements are made in the next few years we may have the satisfaction of knowing that we glimpsed their shadows some time before they arrived, and may even have assisted in hastening their arrival.

DISCUSSION

Mr. Dow: It might be interesting to those here to know that the Dow Chemical Company is turning out a carload a day of fused magnesium chloride at the present time, and is building a plant that will turn out one hundred tons a day. So there will be no shortage. The price after the war went from eighteen dollars a ton up to eighty and is now going back and is around fifty to sixty.

ACID RESISTING ALLOYS.

By W. C. CARNELL.

Read at the Cleveland Meeting, June 16, 1916

The development and progress of the Chemical Industry are vitally dependent on the development of materials out of which the plant equipment may be constructed. A process that is a success in the laboratory, will be a success in the factory, if conditions are duplicated. Platinum and glass are available in the laboratory, but may be prohibitive in the factory. Platinum at thirty dollars per troy ounce was used for the manufacture of concentrating stills for sulphuric acid, platinum at ninety-five dollars per ounce cannot be considered. Glass has its place, but at best is unsafe for extensive processes.

The branch of the Chemical Industry that suffered most for materials out of which to construct apparatus, was the Mineral Acids division.

For many years various metals and alloys were offered for which more or less acid resisting properties were claimed. They had their uses but as complete acid resisting materials they were not successful. With the advent of fused silica a decided advance was made, and it successfully replaced platinum in a number of processes.

In 1911 a basin of fused silica was tried out at the factory with which the writer was connected; so successful were the results that a ten-ton cascade concentrating plant for sulphuric acid was erected to replace a platinum outfit. The results were all that could be desired. The acid made was even better than that made in platinum. Fused silica is brittle and costly and must be handled with as much care as glass.

In 1913 a material under the trade name of "Feralun" was tried out. This was a mixture of cast iron and an abrasive, the abrasive being an alloy of aluminium and silicon. The surface of

the iron was covered with the abrasive. The abrasive was acid resisting, but the acid soon destroyed the iron leaving a porous mass. We, however, found "Feralun" useful for nipples on acid tank cars.

"Fused Silicon." Silicon has acid resisting properties that are all that could be desired for sulphuric acid, but it is more brittle than fused silica, and its use has not been very extensive.

"Ferro Chrome." This alloy is made by the Electro-Metallurgical Co. of Niagara Falls, and is a very promising product. Glacial acetic acid does not attack it, fifty-six and twenty-eight per cent acetic acid attacked it slightly. Strong and weak nitric and sulphuric acids have practically no effect on the alloy. Hydrochloric acid attacked it readily. The experiments were carried out in a bowl eighteen inches in diameter and six inches deep.

Silicon and Iron Alloy. The materials thus far mentioned are valuable for specific purposes. The real dawn of acid resisting alloys came with the use of silicon iron alloys. The beginning of the use of these alloys was about six years ago. The first advertisement the writer observed of this alloy was in the Journal of the Society of Chemical Industry, January 15, 1912. This was of a silicon iron alloy, "Tantiron," put out by the Lennox Foundry Co., Ltd., of London, England.

In May, 1912, after a year of experimenting, the first silicon iron alloy in the United States was put on the market under the name of "Duriron," by the Duriron Casting Co., of Dayton, Ohio. In 1913 the American rights for "Tantiron" was taken over by the Bethlehem Foundry & Machine Co., of South Bethlehem, Pa.

"Ironac" is another trade name for a silicon iron alloy made by the Houghton Co., Ltd., of London, England. While these are all alloys of iron and silicon their composition is not the same.

Silicon iron alloys as put out under the above names is very resisting to all strengths of sulphuric acid, and apparatus made of this alloy is used in all forms of concentrating vessels and cooling devices for the concentration of sulphuric acid. By the use of the so-called Cascade Basins set in a proper furnace full strength oil of vitriol is made from fifty-degree Baumé acid. If all the fittings and coolers are made from this alloy the resulting oil of vitriol is practically free from iron after the plant has been in operation a few weeks. Where brimstone acid is used the resulting oil of vitriol should not contain over 0.0002 per cent

iron. For sulphuric acid concentration, the alloy is durable and the breakage is very small. A plant properly handled will run for months without a shut down. The success of the modern tower system for concentrating sulphuric acid has been due largely to the use of pipes and fittings made of this alloy.

Silicon iron alloy castings have extensively replaced stoneware parts for the manufacture of nitric acid. Early in 1915 the demand for nitric acid increased to enormous proportions; extensions to old nitric acid plants and the erection of new and larger plants was immediately demanded. The capacities of the stoneware factories of the country were soon taxed to their limit. Deliveries could not be made under six months, if at all; had the production of nitric acid been dependent upon stoneware, as it was a few years previous, the production of nitric acid would have been greatly curtailed and the story of the world's war would probably be different.

The silicon iron alloy is resistant to nitric acid of various strengths. It can be cast into all the various forms required for nitric acid apparatus. Castings can be made as readily and as quickly as can those made of cast iron. Here was the ideal substitute for stoneware. Necessity compelled its use and to-day it has largely superseded stoneware for nitric acid production.

A large nitric acid plant equipped entirely with this alloy in the form of "Duriron" was in service for eight months, the alloy showing no indications of corrosion or deterioration. There was practically no breakage. Fire destroyed the building housing the plant and most of the supports for the apparatus. The fire did not harm the castings, though they were exposed to intense heat. Eighty per cent of the castings were recovered and put into service and are in use at the present time. Ninety per cent nitric acid made in a plant equipped with "Duriron" castings showed an average iron content of 0.0014% iron while thirty-six Baumé nitric acid (52.30%) showed 0.0042% iron.

The silicon iron alloy was developed to resist acid. It not only resists acid, but it is resistant to erosion and to rust. Ground surfaces, representing the true alloy are practically immune from rust. The rough castings may show some rust on exposure, but this is due to impurities in the surface, caused by contact of the alloy with the molding sand, this is a surface rust only and will not penetrate. The alloy is also heat resisting, when made of suitable design. The walls must not be too thick for very high

temperatures. Castings do not distort on heating but hold their form up to the melting point.

The following data are given for "Duriron."

Analysis of "Duriron" (approximate):

Silicon	14.00%—14.50%
Manganese	0.25%— 0.35%
Total Carbon	0.20%— 0.60%
Phosphorus	0.16%— 0.20%
Sulphur	Under 0.05%
Melting Point	2500°—2550° F.
Specific Gravity	7.00
Compression strength	70,000 pounds per sq. in.
Tensile strength	25% less than cast iron.

A bar of "Duriron" was compared with a bar of equal size of the best grade of chemical pottery. Under equal conditions, the earthenware test bar broke so quickly that the testing machine gauge did not record any pressure. The "Duriron" bar broke under a load of one thousand pounds. By using a suspended vessel on an earthenware bar and gradually loading it with small pieces of metal and sand, a breaking test of one hundred pounds was obtained for the earthenware bar.

TANTIRON.

Tantiron was first produced by Mr. Robert W. Lennox of the Lennox Foundry Co. of London, England, about 1908. In 1913 the rights for the use of this alloy in the United States, Canada and Mexico were acquired by the Bethlehem Foundry and Machine Co. of South Bethlehem, Pa.

Analysis of Tantiron (approximate):

Silicon	14.00% to 15.00%
Sulphur	0.05% to 0.15%
Phosphorus	0.05% to 0.10%
Manganese	2.00% to 2.50%
Carbon (graphite)	0.75% to 1.25%
Melting Point about	2550° F.
Specific Gravity	6.8
Tensile Strength	6–7 tons per sq. in.

In general, silicon iron alloy cannot be cast in rectangular shapes or flat surfaces. The chemical engineer should collaborate with the foundryman in order to design shapes which can be produced in the foundry, and still serve the purpose of the operating conditions required. The alloy is not suitable for apparatus in which high internal pressures are to be used, unless it is protected by a protecting jacket. Forty to fifty pounds is given as the maximum working pressure for an auto-clave made of Tantiron.

CONCLUSION.

While there is still opportunity for improvement and while there is much more to be desired in an acid resisting material out of which to construct apparatus for the acid industry, yet, the silicon iron alloy or silicide of iron, as it has been called, has proved a boon to the acid industry without which many things could not have been accomplished. It is more efficient than stoneware. At best chemical stoneware if made properly should take ten to twelve weeks for its production. Castings of this alloy can be made and delivered in the same time it takes to make castings out of cast iron. The limitation to castings made of this alloy are those of shop and foundry limitations alone. One company has a foundry with a furnace capacity of seventy-two tons per day. To-day thousands of tons of castings made of this alloy are in use. It is finding its way into all branches of chemical industry. Since its introduction new chemical processes have been started which were impossible before, because of lack of suitable apparatus. Silicon iron alloy is being improved rapidly and the time does not seem far distant when all sorts of vessels will be made of this or a similar alloy that will give to the chemical industry the ideal non-corrosive material that may be fabricated into all the shapes peculiar to the needs of the industry.

DISCUSSION.

Mr. AUBREY: Do I understand that the tensile strength of the stoneware you tested was only one hundred pounds?

Mr. CARNELL: The test was made on a bar. The bar was 24 inches long and was laid on two knife edges and was broken against a bar of the same size made of the silicon and iron alloy, —the only way we could get at it. The knife edges were 24

inches apart. The silicon bar broke with a load of one hundred pounds and the alloyed bar broke under a load of one thousand pounds.

Mr. AUBREY: I would like to state that we make various kinds, and qualities of stoneware and the lowest we make has a tensile strength of twelve hundred pounds and the highest of twenty-four hundred pounds.

Mr. CARNELL: It is really a test of the breaking strength that was made, a transverse strength test. Transverse tests on Duriron were made several years ago, with the same size bar and the average breaking load was somewhere between 900 and 1000 pounds—tests on individual bars varied considerably. Bars were made up of the best imported earthenware, of the same size, and when these were tested at the Board of Water Supply Laboratory, they broke so quickly that no reading could be made—they were then supported on 24-inch centers and a bucket of water suspended in the middle; into this was put slowly and carefully small pieces of iron and under such conditions the earthenware finally broke at 100 lbs.

CHAIRMAN: I should think there are some among us who are particularly interested in acid resisting alloys and who have had experience. We are very desirous of hearing from them.

Mr. MASON: May I ask if Duriron can be machined or ground?

Mr. CARNELL: Duriron can be ground and machined; it is ground in order to make joints and fittings.

Mr. BARTON: Isn't it a fact that as soon as the Duriron or Tantiron is ground or machined that the removal of the surface seems to permit corrosion to take place?

Mr. CARNELL: No, absolutely not. The fresh surface is more resistant than a surface after casting.

Mr. BARTON: I understand as soon as the surface had been attacked to any extent it became very rough?

Mr. CARNELL: That has not been our experience. We used these alloys at first simply because we could not get stoneware. Now we would not go back to stoneware.

Mr. MARSH: Will you tell us what the difference is between Tantiron and Duriron, why one can be machined and the other can not?

Mr. CARNELL: It is the difference in the percentage of manganese and carbon.

Dr. ANDREWS: I would like to ask Mr. Carnell to what extent the resistance of the silicon irons is dependent upon the character of the acid, whether their resistance to acids other than sulphuric is at all comparable to their resistance to sulphuric; whether they will stand up against phosphoric acid, or even organic acids, and to what extent?

Mr. CARNELL: It will stand acetic, lactic, nitric and sulphuric acids. It will not stand up against hydrochloric acid. It will resist phosphoric acid under certain conditions very well, under some conditions there is very decided corrosion. Phosphoric acid made from rock is apparently more corrosive than that made from bone, probably due to the minute quantities of hydrofluoric acid present.

CHAIRMAN: Mr. Carnell spoke primarily, I believe, of the strong acids; of course, it is quite often a serious problem to have an alloy that will resist the action of weaker acids.

Mr. CARNELL: They will resist the action of sulphuric and nitric acids of any strengths.

Mr. PHILIPP: What is the action of phosphoric acid? The object of my question was more particularly to get some information, if possible, as to how they stand up against organic acids. My own experience in regard to phosphoric acid is entirely in accordance with that of Dr. Andrews.

Mr. CARNELL: It is entirely possible to use them for phosphoric acid. Both of these alloys are used for anodes and cathodes.

Mr. BURNS: Mr. Chairman, I would like to ask Mr. Carnell whether or not he has made any observations regarding the temperature conditions of the castings of Duriron and Tantiron, as to the effect of acids on the iron at different temperatures?

Mr. CARNELL: As far as I know hot acid seems to attack it even less than cold.

Mr. BURNS: I would like to add just two points: My experience has been that Duriron stands up very well in high ferric acids; but the main point is that it is so hard and brittle you are confined to jewelers and watchmakers in order to handle it; it is extremely brittle, even more so than pure silicon, and it seems to me too brittle for practical use. Another point, which perhaps is more an engineering difficulty, is that, as the centrifugal pump is constructed one has great trouble to pack it, the lining does not

seem to be made deep enough. That, however, could be remedied by changing the design so it would take more packing.

CHAIRMAN: This discussion seems to be developing into a very useful one, and I would not like to cut it short, but rather encourage all members to give us the benefit of their experience.

Mr. CARNELL: In reply to Mr. Burns. We have had some experience with these pumps. When we first started to use pumps of this alloy, we had a great deal of trouble, but after a while we got used to handling it and our troubles disappeared. We have a centrifugal pump, pumping hot acid, it has been running two months now, we have had practically no trouble with the pump. You must find a suitable packing.

Mr. BURNS: What kind of packing is it that you use?

Mr. CARNELL: It is a special packing recommended by the makers of the pump, and it holds out all right. Our experience in dealing with these alloys has been both with pumps and cocks, that when first used there is more or less trouble but as the men get used to handling them the trouble disappears. I would like to add that these alloys are not nearly so brittle as pure silicon, we have handled both.

CHAIRMAN: I take it, this discussion is not limited to Duriron in particular.

Mr. MASON: Mr. President, I want to ask if he has had any experience with Monometal, as being acid resisting?

Mr. CARNELL: Yes, our experience was very disastrous. We did not get results, especially on valves. The metal did not hold up.

Dr. ANDREWS: If there is no objection to my taking the floor again, I would like to speak in connection with non-ferrous alloys. I have made the observation, which may be interesting to some of our members, that the heat treatment of those alloys has a very great influence on their acid resisting properties. The brasses and bronzes, after rolling or hammering so that they are in the hard, spongy condition, have a very much greater resistance to the action of acids, than they had after being softened by heating and plunging in cold water; and that is not due to any chemical action can be shown by the fact that the same specimen which has had a very inferior resistance in its soft condition, by being hardened again by hammering shows its original resistance and the same experiment can be repeated again and again with the same results. This is the case with the aluminium bronzes and

bronzes containing aluminium and phosphorus, and bronzes consisting mainly of copper and tin with very small amounts of phosphorus and lead. I might say that we tried a series of tests of the corrosive action of zinc chloride solutions saturated with ammonium chloride on Duriron and found that Duriron was extremely resistant and gave very little corrosion whatever. In the chlorine series of tests we immersed the Duriron in a zinc chloride solution completely, weighing at the end of every week for four weeks in succession and found no serious corrosion. In another test, I immersed for one day and allowed it to stand in the air for another, and did that for three weeks in succession and found no serious corrosion.

CHAIRMAN: I noticed a few minutes ago one of our members with much experience in the manufacture of acids enter the room. I would like to ask Dr. Lihme whether he has had any experience with reference to Duriron or any other acid resisting alloys?

Dr. LIHME: I just came in the room a few minutes ago and was not here when the paper was read and heard but little of the discussion; but as to Duriron I would say that we use it and it will resist acids all right, but we cannot use it for heating because it is too brittle. Otherwise the resistance is great, and it is a good material for many purposes.

CHAIRMAN: Mr. Carnell, do you wish to reply to Dr. Lihme? I thought I saw you rise.

Mr. CARNELL: Yes! Dr. Lihme is rather too general in his statement that Duriron will not resist heat because it is brittle—we have apparatus heated to moderate temperatures and some to high temperatures—for successful use under heat conditions the walls must not be too thick, and the apparatus must be designed accordingly. I would further like to ask Dr. Lihme what his experience has been with muriatic acid and this alloy?

Dr. LIHME: I never tried the muriatic acid in our place because I did not believe the alloy would stand up.

A STUDY OF THE EFFECT OF STORAGE ON MIXED PAINTS.

E. E. WARE and R. E. CHRISTMAN.[1]

Read at the Cleveland Meeting, June 16, 1916

The manufacture of ready mixed paints is a development of comparatively recent years. Pigments ground in oil to be mixed and tinted by the individual painter were much more commonly used than ready mixed paints twenty-five years ago, while it was not at all uncommon practice for the practical painter to mix the dry pigments with oil. It may be possible, if proper precautions be used, and enough time be taken, to prepare a satisfactory paint in this way, but extensive investigation has shown that it is much better to grind at least part of the oil with the pigment. The painter to-day has neither the necessary skill nor the unlimited patience required for mixing the dry pigments with the oil, even if it were possible to compound in this manner as satisfactory a product as the paint manufacturer with his years of experience and his intimate knowledge of materials is able to produce by extensive grinding and an intelligent proportioning of the different ingredients.

One of the earliest difficulties encountered in the manufacture and use of ready mixed paints was the tendency of the heavy pigments to settle away from the vehicle if the paint were permitted to stand any time before use. It was soon discovered, however, that the addition of a small amount of water containing a suitable emulsifying agent helped to prevent this settling of the pigment. But while eliminating one source of difficulty, the practice has brought on other troubles, a satisfactory remedy for which has never as yet been advanced.

Some emulsified mixed paints, if left to stand for several years, will show evidence of a deterioration which manifests itself

[1] Holder of the Acme White Lead and Color Works Fellowship in Chemical Engineering at the University of Michigan. 1915-16.

in one of two ways, either through the formation of an amorphous soapy blanket between the settled pigment and the supernatant vehicle, or by the gelatination of the entire paint to a mass of about the consistency of soft putty, the vehicle seemingly having entirely disappeared. The first case is usually spoken of as "skinning" and the second as "puttying."

These phenomena have given the paint manufacturer much trouble, and consequently have been the incentive for considerable experimental work in an effort to compound a paint formula that will show them to a minimum extent. While it has been quite generally acknowledged that neither trouble will occur in the absence of water, it has been felt that water exerted only a secondary action, the principal cause of deterioration lying in some action between the pigment and the oil of the vehicle. Most of the published data have been compiled as a result of investigation along this line.

In 1911, Gardner,[2] in studying the possible effect of pigments upon the paint vehicle, ground the more common ones, both the active and the inert, in raw linseed oil and compared the ash content of the oil before grinding and after it had remained in contact with the pigments for a month. The oils from the mixes containing white lead, zinc oxide and red lead showed an appreciable increase in this ash value, and Gardner advanced the opinion that these pigments have the power of saponifying the oil with the formation of their respected soaps. He stated that the inert pigments have little action by themselves and when mixed with active pigments tend to retard the action of the latter. A few months later, Sabin[3] published an answer to this in which he questioned both of these theories.

In a later publication, Gardner[4] described the effects of pigments upon linseed oil after a storage of two years. Various pigments were ground in a good raw oil without drier or thinner, using a sufficient amount of oil to bring the mixture to the desired consistency. The most marked effect on the oil was the change in its iodine number which was lower in all cases after storage. The oil of the red lead mix showed a final iodine number of 135.4, while in the other cases the value had dropped to approximately 160 from an original value of 181. The ash content

[2] Jour. Ind. and Eng. Chem. 3, 628, 1911.
[3] Jour. Ind. and Eng. Chem 8, 790, 1911.
[4] Jour. Frank. Inst. 174, 415-423, 1912.

of the oil was affected the most in the paints containing white lead, red lead, and basic chromate of lead, but to a considerable extent also by both carbon black and graphite. He concludes that the perfect condition of these experimental mixtures warrants the manufacturer in assuming that any properly prepared combination pigment paint may be safely stored in cans for long periods without bad effects, provided that the oil used be of normal grade. He also concludes that the action of the free acids upon the pigments was the real cause of the hardening.

Boughton's results were somewhat at variance with those of Gardner in that they did not show a change in iodine value of more than ten points in any case, while the corroded white lead sample was the only one in which there was any marked change in the ash content of the oil.

In a more recent publication, Gardner states[5] that the oil in paint is hydrolyzed by a fat splitting enzyme, lipase, produced in seeds of the oil-producing type. He claims that the seed is not always steamed before expression to a temperature that will kill all of the micro-organisms, and spores which pass into the oil soon develop the enzyme. He further states that the fatty acids formed are partially neutralized by the basic pigments whereby lumps of an insoluble fatty acid soap are formed. He recommends heating cloudy raw oil to at least 100° C. before shipping, in order to kill all micro-organisms.

EXPERIMENTAL.

In considering this important problem, the authors felt that it might be possible to get a clearer idea of the underlying causes of these deteriorating influences, if paint specimens in which the action was pronounced could be procured, and the different products contained therein isolated and examined. Accordingly an effort was made to collect a number of samples of standard commercial paints which had been in storage for considerable time. Those actually used in this investigation had been on the shelves for from two to eight years.

The blanket effect or "skinning," which was very pronounced in some of the samples, was studied first. The paints in which this had occured were all composite base paints, which had

[5] Jour. Frank. Inst. 177, 533, 1914.

originally contained a small amount of water, while one also contained a considerable proportion of gloss oil.

The oil, extracted from the pigment and with the thinner completely distilled off, showed an average iodine number of 181 and an acid value of 15. The ash content was low, running about 1.5 per cent and consisting mainly of lead oxide, with some manganese and zinc. The recovered oil was clear and of good color.

Samples of the skin were repeatedly washed with petroleum ether until all adhering oil and pigment were removed. The composition of this skin was variable, the weight of the oxides ranging from 13 to 16 per cent; the average of a number of samples being about 14 per cent. The proportions of the various oxides varied within narrow limits; but a typical analysis, corresponding to 14 per cent oxides, showed:

PbO	12.0
ZnO	87.0
R_2O_3	1.0

The zinc oxide was similarly predominant in all samples analyzed, indicating that it was the active agent in the formation of the skin.

The free acids were liberated from the skin by heating with a dilute solution of mineral acid without access of air, the fatty acids dissolved in ether, and the ether solution evaporated under vacuum.

In the first experiments, hydrochloric acid was used as the liberating agent, which procedure gave free fatty acids exhibiting the following constants:

Neutralization value	166–167
Saponification value	203–205
Iodine value	170–178

Such abnormal differences between the neutralization and saponification values clearly show, according to Browne,[6] the presence of an unsaturated lactone. The amount of this lactone was estimated according to the method of Lewkowitsch[7] and found to be 20 per cent of the total acid. As the purified

[6] Jour. Ind. and Eng. Chem. 7, 30, 1915.

[7] Chemical Technology of Oils, Fats and Waxes, 1, 423.

lactone was liquid it hardly seemed probable that it was formed as a product of reaction during storage of the paint, for in that case it would have remained in solution in the oil rather than have been precipitated in the skin. It was no doubt formed during our manipulation when the fatty acid was freed from the zinc soap, the action being accelerated by the presence of the resulting zinc chloride which acted as a condensing agent. Zhukov and Shellakov[8] studied a similar reaction and came to the conclusion that zinc chloride has the power of throwing the double linking along the carbon chain until it reaches the gamma position where it is converted into the lactone. Concentrated sulphuric acid exerts a similar influence, although the dilute acid has no action.

The acids of the skin when liberated by dilute sulphuric acid, proved to be the usual linseed oil acids having the following constants:

Neutralization value	193.5
Saponification value	203.0
Iodine value	181.0

This remaining difference between the neutralization and saponification values showed that the free acids still contained some lactone, probably due to the condensation of a small amount of gamma hydroxy which may have been present, and which on liberation changed immediately to the lactone.

The Gusserow-Varrentrapp or lead salt ether method for the separation of solid and liquid fatty acids showed a solid acid content in the skin of 10–12 per cent, a figure not inconsistent with that of 7.5 per cent ordinarily given for raw linseed oil.

Samples of the skin were dissolved in hot alcohol-ether solution, and gave upon titration an apparent neutralization value as high as 168. This at first suggested the possibility that the skin was a product of the coagulation of a colloidal suspension, but experiments to establish this failed to yield definite results and were abandoned.

On extracting the skin with hot absolute alcohol, the entire mass was dissolved. On cooling, a white flocculent material composed of metallic soaps and hydroxides was precipitated, while the residue in the filtered solution, upon evaporation of the alcohol,

[8] J. Russ. Phys. Chem. Soc. 40, 830–9.

was recovered as free acids. An extraction with ether failed to show the presence of these free fatty acids and it was decided that during the extraction the soaps had been hydrolyzed. This same condition undoubtedly existed when the skin was titrated with dilute KOH in determining its acid value, which would account for the high value obtained. Weaker hydroxides Ba $(OH)_2$ and NH_4OH acted similarly, as did an absolute alcohol solution of KOH. To check these observations, a pure zinc soap of linseed oil was prepared. This soap exhibited the same tendency to hydrolyze giving an apparent acid value indicating complete hydrolysis as figured from the formula of zinc linoleate.[9]

Since in the skin or blanket of a mixed paint, the metallic soaps present are mostly all those of zinc, which are completely hydrolyzed in analysis, the determination of the acid number of this material does very little toward establishing its constitution.

However, it seems reasonable to assume that the skin consists almost entirely of the neutral metallic soaps of zinc and lead, for any free acid present should be perfectly soluble in the oil which is in contact with the skin. Also if the total weights of the zinc and lead soaps be calculated from the oxides contained in the ash as given above, using 275.5 as the average molecular weight of the fatty acids, they are found to check up to the original weight of the sample taken for analysis.

It is not probable that these precipitated metallic soaps that go to make up the skin result from the saponification of the neutral glycerides by the pigments alone, although at different times writers have stated that white lead has the power of saponifying the oil to form a lead soap. Such reactions may occur at high temperatures, but it is not probable that they proceed to any extent at ordinary temperatures unless influenced by some other agency. The results obtained by Gardner and Boughton do not necessarily signify that even the slight changes in the characteristics obtained by them are due entirely to the pigment, since the action of any water present must be taken into consideration.

It is possible that the accelerator necessary to this metallic soap formation is lipase, as claimed by Gardner, but it is the opinion of the writers that, although the action of lipase may at times be considerable, nevertheless it is of secondary importance in the reactions taking place in the ordinary mixed paint. The

[9] Jour. Ind. and Eng. Chem. 8, 996.

occasional presence of lipase, the fat splitting enzyme, in linseed oil has long been established. Zymogen, in the flax seed, under favorable conditions, is capable of forming the enzyme, and it is of real importance in expressing the oil that the seeds be heated high enough to kill any micro-organisms present, not only to prevent the formation of lipase in the oil, but also to insure its absence from the press cake. The enzyme has the ability to act upon the cyanogenetic glucoside, linamarin, present in the cake, and with the aid of water to convert it into prussic acid, making the cake entirely unfit for cattle food.

However, it does not seem probable that the presence of a fat splitting enzyme in linseed oil, is a frequent cause of trouble. Even when using cold pressed oil, trouble arising from the action of lipase would not be common, for, as shown by Tanaka [10] the enzyme lipase is inactive in the presence of even dilute alkali, a material which is commonly present as a constituent of the commercial emulsifying agent.

As a further evidence of the exceptional rather than common occurrence of this enzyme in linseed oil, cultures were made according to the method recommended by Gardner, using several different commercial oils and one sample of foots. In no case was any growth apparent after 72 hours.

The authors believe that the really important hydrolyzing influence is the emulsifying agent. The sodium hydroxide formed in the solution, has the power of saponifying linseed oil at ordinary temperatures, while the sodium soap so formed acting as an emulsifying agent, tends to keep the water and oil in contact, giving a better opportunity for further action.

The alkali soaps are at least partially hydrolyzed in water solution, and as the acid formed is soluble in the oil present, there may be a partial recovery of the alkali which furthers the reaction in forming more soap, so that we might say that the governing factor in establishing the limit to which hydrolysis will proceed will be the amount of water present.

When ready mixed paints are stored, the first general reaction is this hydrolysis of the oil by the water, accelerated by the presence of the alkali, and perhaps affected to some extent by the pigments. This hydrolysis is necessarily slow since the water and oil are immiscible, and before any large amount of acid is formed,

[10] J. Coll. Eng. Tokyo, 5, 1912.

the pigment has settled leaving the clear vehicle above. The acid coming in contact with the top of the settled pigment, reacts with the basic materials present of which zinc oxide is the most active, and forms the corresponding metallic soaps. Originally two molecules of water had hydrolyzed the oil and formed two molecules of the acid, and upon the reaction between these two molecules of acid and the metallic oxide, one molecule of water is liberated. In computing the amount of hydrolysis under such conditions, it may be shown that with an amount of water such as is sometimes present in mixed paints, it is possible to get complete hydrolysis of all the oil present. This is a condition that does not occur in the average paint, nevertheless the speculations are not greatly overdrawn, and the possibility of injury to mixed paint by the use of improper emulsifying agents is clearly shown.

The zinc soap formed on the neutralization of the acid by the zinc oxide of the pigment is almost completely insoluble in the cold vehicle and is at once precipitated on top of the partially settled pigment. The white lead also unites with the acid, with the formation of the more soluble lead soap, but which is partially carried down by the zinc, and so helps to build up the peculiar skin over the pigment. The reactions between the acid and the pigments take place readily so that the oil never shows a pronounced free acid value. This value previously cited as approximately 15, when allowance is made for the difference due to the soaps dissolved and hydrolyzed during analysis, is corrected to about eight.

Some of the water is probably carried along with the settling pigment so that the same reaction takes place there. The metallic soap formed in the midst of the pigment may be the cause of the non-caking of the pigment in stored mixed paints. The opportunities for reaction in the presence of the great excess of pigment are very good, and the tendency is in favor of the formation of the basic soap rather than the neutral soap as present in the skin. Upon extraction of the settled pigment with ether, the presence of the basic soap was inferred from the fact of its relatively slight solubility. The extracted oil was clear and of good color, with an apparent acid value of 16.8. Upon correction for the ash content of 1.16 figured to hydrolyzible metallic soap, the true acid value would be about 10. The pigment residue from the extraction still contained some fatty acid compounds,

the freed fatty acids checking to 2.14 per cent. These were probably present as the basic soaps and would represent a weight of 2.76 per cent if figured as the basic zinc soap and 3.8 per cent if figured as the basic lead soap.

As a further evidence of the plausibility of the above analysis of conditions, it is possible to synthesize the zinc oxide fatty acid blanket if a small amount of oxide is allowed to stand undisturbed in contact with oil containing a large amount of free linseed oil acids. If one gram of zinc oxide is placed in the bottom of a flask and covered with acid, within 24 hours the zinc oxide will swell out from conversion to the soap, giving the characteristic lumpy appearance of the "skin" frequently seen on paints.

The zinc soap is quite insoluble in oil or naphtha, though soluble to an appreciable extent in turpentine. The insolubility of the soap may explain why the constants of the oil in the zinc oxide linseed oil storage tests conducted by Gardner and by Boughton showed no marked changes.

If a small amount of white lead be treated with excess acid in the same manner as succeeded in developing the linseed zinc oxide skin, we find that the action is much slower and several weeks' time is necessary before there are any definite indications of reaction. The lead soap formed is of a finer structure and does not show the same tendency toward coagulation or lumping.

PUTTIED PAINT.

The similarities in the reactions giving a puttied paint to those just reviewed is quite apparent. Several cans of the worst cases of puttying were studied, in which the entire liquid portion had seemingly gone solid to a dense soapy mass. These paints were of a grade inferior to the paints exhibiting skinning, in that they contained more water as well as considerable rosin in the form of gloss oil.

The oil, extracted from the paint and with the thinner distilled off, was clear but slightly dark in color. The analytical constants of the oil were as follows:

Apparent acid value	33.0
Saponification value	177.0
Iodine value	184.0
Ash	4.5%

An analysis of the acids of the oil by the Twitchell volumetric method indicated the presence of 14 per cent of rosin acids. The ash consisted mainly of zinc oxide with some lead and lime. These oxides were present as a mixture of linoleate and resinate, a separation of which was not feasible. Assuming the same extent of hydrolysis in the determination of the acid number as had been proven in the cases of these salts previously, the free acid content was shown to be negligible.

In these puttied paints, the pigment had settled to some extent and it was found convenient to work with the upper layer which was puttied vehicle containing a comparatively small amount of pigments. This was quite variable in composition, the percentage of free pigment increasing on approaching the settled body of pigments. The amounts of free pigment and of zinc soap being the important determinations, these were ascertained within sufficiently close limits by shaking the sample with cold ether, weighing the residue, liberating the acids in this residue and calculating the zinc linoleate from these acids. The zinc soap of the oil is almost entirely insoluble in cold ether while the resinate of zinc is quite soluble under the same conditions, and since the samples taken were in themselves quite variable the results were as reliable as could be expected. Near the top of the puttied vehicle they constituted 10–15 per cent of the total and the zinc soap 20–25 per cent. Deeper in the layer the pigment increased to 25 per cent while the zinc soap content decreased to 15–20 per cent. The relative amounts of oil, thinner and resinate of zinc were not determined in these samples. The pigments separated from these samples analyzed about 75 per cent zinc oxide.

If a small amount of zinc oxide be allowed in contact with raw linseed oil containing rosin dissolved in petroleum thinner, in a very short time the oxide dissolves to a clear solution. The only apparent change in the solution is an increase in the viscosity. The same condition holds when free oil acids are substituted for the raw oil. There is no tendency toward the formation of the skin; in fact a sample of the skin previously formed dissolved completely in a naphtha solution of rosin upon standing over night.

The assumption is that the rosin acid displaces the oil acids from their zinc soaps with the formation of zinc resinate, which is somewhat soluble in naphtha, and considerably soluble in turpentine, ether, and linseed oil.

The paints containing gloss oil seem to be the most susceptible to livering, which may be explained as follows. Although the rosin is usually limed there still remains considerable free rosin acid whose combination with basic pigments takes place in a short time. The resinates so formed are soluble in the vehicle but increases its viscosity to a point that delays the settling of the pigment. The hydrolysis of the oil proceeds as in the case of the skin formation except that as there is generally more water and more alkali present the action is faster. The freed acid comes into immediate contact with the unsettled pigment forming the metallic soaps especially that of zinc. The vehicle portion becomes constantly thicker until we have a condition approaching that of colloidal gel formed from the finely divided zinc soap precipitating from the viscous solution of zinc resinate. This gel has occluded or absorbed the remaining oil and what little thinner may have been present. Since, during the progress of the reactions, the acids and the pigments are in intimate contact the opportunity for neutralization is much better than is the case with the skinned paints, which accounts for the low free acid content of the extracted oil. If the rosin of the gloss oil be highly limed or for any other reason the pigments should settle before the oil acids form to any extent, the action is apt to be one of "skinning" rather than "puttying."

An experimental mixture of oil, acids, rosin, and petroleum thinner resulted in a livered paint, which although not so hard as the samples investigated, was in every way comparable to them.

CONCLUSIONS.

The use of emulsifying agents in paint grinding is quite generally recognized as necessary, although their action in preventing the hard settling of the pigment has not as yet been satisfactorily explained.

Certain of the emulsifying agents hydrolyze the oil of the paint, the fatty acids subsequently forming insoluble zinc and lead soaps, which if free to precipitate form a skin on the surface of the settled pigment.

If because of the presence of dissolved resinates formed from added gloss oil, or for any like reason the viscosity of the vehicle be increased to the point where the pigments and the metallic

soaps formed from the hydrolyzed oil are not free to settle, there is formed a gel or "livered" vehicle.

These conditions may be remedied by the use of a protective colloid containing no water, such as aluminum oleate or palmitate, or by the use of a water containing emulsifier in which the active agent exerts no saponifying action on the linseed oil.

Experimental formulas compounded in an effort to establish the relative values of various emulsifying agents although they show promise, have not as yet been under observation for a sufficiently long time to warrant definite conclusions being drawn from them.

University of Michigan,
Ann Arbor.

DISCUSSION.

Dr. ALLERTON S. CUSHMAN: Prof. Ware's paper discloses the results of some highly interesting research work. It is probable, however, that he has placed too much emphasis on the conditions which result upon the storage of emulsion paints. This is due to the fact that the majority of mixed paints now put up by manufacturers contain neither emulsifying agents nor added water, and the phenomena referred to should not, therefore, be considered as a common evil.

It is gratifying to note that Pro. Ware has recognized the possible effect of enzymes in splitting up linseed oil into free fatty acid, the substance that may be considered as the real cause of all the trouble. It is probable, however, as my associate, Gardner, has previously pointed out, that storage troubles in paint will not develop to any substantial extent, provided the oil used has been heated, is of low acid value, well settled, clear, and free from foots and moisture.

Mr. LUTES, of the Sherwin-Williams Company: I am associated with Mr. Holton, the Chief Chemist of the Sherwin-Williams Company, and he wasn't able to be here this morning and asked me to attend the session. I would like to read a letter from Mr. Holton, written after we had looked up some samples of paint that were from twenty to twenty-five years old. We found quite a number of samples and I brought some of them with me this morning. This letter is from Mr. Holton and is addressed to

Mr. Ware and expresses our findings as well as we could after looking over these results, based upon our experiences of the past.

Letter read as follows:

"I wish to thank you for sending me advance copy of your paper on 'A Study of the Effect of Storage on Mixed Paints,' and sincerely regret my inability to attend the session at which you will present it.

The problem of preparing paint which will remain in the package for a long period of time without undergoing any chemical change and will upon application immediately begin to pass through changes which bring about setting up and drying satisfactorily in a short space of time (usually a few hours) is always present to the manufacturer of ready-for-use paint. Although, perhaps, little has been written on the subject it has been generally known that paint vehicles containing free fatty and resin acids when mixed with pigments such as zinc oxide, basic carbonate of lead, basic sulfate of lead, alkaline whiting, aluminum hydrate, lakes and many other alkaline or strongly basic pigments mutually react to a greater or lesser extent, the resulting product often becoming quite thick or even solid in some cases.

It is also well known to the paint manufacturer that paint which will withstand these storage changes almost indefinitely may be made by using neutral linseed oil with zinc oxide and basic carbonate of lead and many other artificial pigments, also many of the pigments found in nature.

Our experience would seem to indicate that the presence of water in noticeable quantities is not needed to bring about the storage changes referred to. Of course, water is always present in minute quantities since neither pigments, oils nor spirits of turpentine are strictly anhydrous. Water present in considerable quantities may sometimes act as an accelerator when alkali or pigments having a strong alkaline reaction are present and especially when resinous dryers, reducers or varnishes are present.

In general when dryers and varnishes are prepared from various resins, oil and volatile solvents, the resinous acids present are converted in the melting kettle into the anhydride form. These anhydrides have a tendency to take up water, forming compounds which are much less soluble in the ordinary solvents, hence the tendency to blanket and liver.

Partial oxidation of linseed oil produces compounds of much lesser solubility in paint thinners, especially in petroleum distillates and it may easily happen that a mixture which to-day seems to be a perfect solution is in reality almost a gel and will when used in paint mixtures begin to liver or blanket as the other conditions may determine.

It seems probable that the following are the most important changes taking place in storage.

Formation of less soluble metallic soaps by reaction between basic pigment and fatty and resinous acids.

Precipitation of oxidation products from gels.

Formation of insoluble resinous acids by hydration of resinous anhydrides.

Formation of insoluble compounds resulting from reactions in which manganese dryers act as oxidizing agents and are themselves reduced.

Changes in which Prussian blues, etc., pass into the colorless condition, the color reappearing to a greater or lesser extent on exposure to air.

Changes in which Chrome Yellows are partially reduced and other serious color changes occur due to oxidation or reduction according to circumstances."

If you have time, I wish you would look at these samples. I will be glad to show them to you.

Dr. ITTNER: While I am not an expert in this line, in the paint line, I would like to offer a suggestion which occurred to me during the reading of these papers, which may be something in the way of an explanation.

Professor Ware said that this precipitation, called skinning, occurs more readily when there is some anhydride like caustic soda or carbonate of soda present with water; and this other letter indicated that it occurs only when a very small amount of water is present. It seems to me that the logical explanation—it may not be the correct explanation—is that we know that caustic soda in the presence of water or neutral oil will under many circumstances form a small amount of soap, and this soap formed is more or less soluble in water. There is a tendency well known—Berthollet's Law is what I refer to—that when you have a soluble substance and another substance which is not, it will tend to make the more insoluble substance. You have this

soluble soap present and the zinc oxide, carbonate of lead or something which will tend to make a more insoluble substance present, and then you have a shift from the soda soap, which is first formed, to a more insoluble zinc soap or lead soap, which immediately sets forth the original caustic soda which is there to saponify a further portion of the neutral oil, and the tendency would then be for the thing to react and form more of the insoluble zinc soap, if that is a basic, or a more insoluble lead soap. If the basic soap is more insoluble than the neutral lead soap, the tendency would be for that to form; and where the insoluble soap is more insoluble than a soda soap but is still somewhat soluble in the vehicle, the tendency would be not for it to precipitate out as an absolutely insoluble precipitate like the skin, but to remain more or less dissolved in the whole vehicle and form a gel which would be the result of the puttying. I do not know that this is so, but from my knowledge of facts, I think that might be a partial explanation.

CHAIRMAN: I am afraid we will have to close the discussion as it is now twelve-thirty. We will stand adjourned until to-night.

WATER POWERS OF THE WESTERN UNITED STATES

By HERMAN STABLER,
Hydraulic Engineer U. S. Geological Survey

Read at the Cleveland Meeting, June 16, 1916, by permission of the Director, U. S. Geological Survey

THE UNITED STATES AS AN OWNER OF POWER SITES

The Federal Government is the sovereign proprietor of some three or four million acres of land west of the Mississippi River valuable chiefly for the development of hydroelectric power. These lands have so far been used in connction with the development of energy to the extent of about one and one-third million horsepower of installed capacity, and it is estimated that the installation is likely to reach about thirty million horsepower without resort to extensive conservation of stream flow by storage. Perhaps half of the ultimate water-power development in the United States will involve the occupation of what are now public lands.

It is the duty of Government officials, in pursuance of legislation enacted by Congress, to encourage the utilization of power lands in such manner that the greatest public benefit will result. Under present policy such lands are withdrawn from general entry under the agricultural land laws and are held for use under the act of Congress providing for power development. It should be clearly understood that the withdrawal in such cases is not a withdrawal to prevent the development of water power but one to facilitate it. It is true that under present laws it is not possible to give a tenure of public lands for power purposes as satisfactory as could be desired, and this may have had a slight retarding effect on power development. But millions have been invested on the basis of this tenure without loss on account of it, and where conditions of economic development and satisfactory market for energy exist no retarding effect of insecure tenure of Government regulations can be observed. In fact, cases might be cited in which capital has been

and is being invested in large amounts for water-power development on public lands without permission of any kind.

The United States Geological Survey, as a part of its work of classification of the public lands, is charged with the duty of determining what lands are valuable for water-power purposes and for the deposits of minerals which they contain. The data here set forth have been taken from the material accumulated in connection with this work of classification and relate for the most part to proposed developments involving the use of lands of the United States.

POWER FOR THE ELECTRO-CHEMICAL AND ELECTRO-METALLURGICAL INDUSTRIES.

Cheap power is desirable, though not necessary, for all electro-chemical and electro-metallurgical processes. The cost of power is merely one of half a dozen factors that may determine the success or failure of an industrial establishment that uses great quantities of electric energy. The cost of raw materials, the cost of labor, and the cost of marketing the product are all material factors in the economic problem. So far as power is concerned, the West has a number of water-power sites at which energy can be developed as cheaply as at Niagara, and a few that are comparable with Niagara in size. There are some localities where cheap power and abundant deposits of the raw materials of certain processes are located in proximity. But labor is generally expensive, freight rates are high, and the West itself provides but limited market for electro-chemical products. It is an exceptional industry and a wonderful water-power site that will warrant the cost of transporting raw materials to the West, manufacture under adverse labor conditions, and the marketing of the finished product near the Atlantic seaboard. Too much should, therefore, not be expected of the West as wonderful as its water-power resources are. There should be a steady development to supply the markets of the West and with respect to some industries there is reason to believe that it may be possible to market finished products in the East in competition with those developed in connection with the better eastern water powers. This will be increasingly true as the surplus of cheap hydroelectric power in the East becomes exhausted.

Nowhere in the United States are there water-power sites so

favorable for electro-chemical developments as some of those in Norway. But the cheapest of the Norwegian powers is now developed and the United States should be able to supply its own markets at least in competition with hydroelectric developments that may hereafter be made in Norway.

It is estimated that steam-electric energy can be sold in large blocks under a high load factor for between $20 and $25 per horsepower year if the power plant is located at a coal mine and the distance of transmission is negligible. In a general way, it may be stated that the selling price per horsepower year of hydroelectric energy in large blocks under high load factor at the power plant should be about one-seventh the development cost per continuous horsepower of the plant capacity, if proper allowances for operation and administration, maintenance, repairs, depreciation, and return on investment are allowed. Competition with cheapest steam power will be met, therefore, if other conditions are on a parity, by water powers that can be developed at a cost of about $150 per continuous horsepower. This will not provide energy cheaply enough to encourage many electro-chemical industries. If, however, the hydroelectric development can be made for less than $100 per continuous horsepower, energy can be sold at an attractive figure for electro-chemical purposes, always provided distance of transmission is not great, raw materials are cheap, and market conditions are reasonably favorable.

In general power sites can be best used when developed as parts of extensive systems including many varieties of service. Small electro-chemical industries can perhaps best be built up as a part of the power market of such systems. In the case of establishments using very large amounts of power a hydroelectric development can sometimes be made for a single industry, and a general power system thereafter be built up around it if surplus energy is available. Discussion of power sites in the West with respect to electro-chemistry and electro-metallurgy may therefore properly relate to the large power systems that have been developed and also to undeveloped power sites of considerable capacity that are not held under the control of public utility corporations.

DEVELOPED POWER

Developed water power in the Western States is very largely controlled by a few power systems. The following systems are of sufficient importance and have a sufficient reserve of well-located undeveloped water power to be of special interest:

1. Puget Sound Traction, Light and Power Company, and its subsidiary and allied corporations controls the general power business in northwest Washington. It is a Stone and Webster corporation. This company states that it has hydroelectric plants and sites with a present development of 74,000 horsepower and an ultimate development of 175,000 horsepower. Stone and Webster interests also control, or at least have a material hold, upon sites with additional capacity of more than 100,000 horsepower. This undeveloped power merely awaits a suitable market. The sites are capable of development at relatively low cost.

In a recent study for increased power for the City of Seattle it is estimated that there are six undeveloped power sites within 100 miles of Seattle which could be constructed for capacities of 50,000 to 160,000 horsepower at costs, including transmission lines, of $75 to $110 per horsepower.

It is questionable whether any of the undeveloped water-power sites in the Puget Sound region could be profitably developed for the exclusive use of electro-chemical industries.

2. The Washington Water Power Company, with headquarters at Spokane, supplies the power market of southeast Washington and a part of Idaho. This company has installed water-wheel capacity of 85,000 horsepower in four plants and claims undeveloped water-power capacity of 58,000 horsepower in addition. There are other well-located sites in the territory covered by the Washington Water Power Company to which it lays no claim.

3. The Montana Power Company and its subsidiary and allied corporations has a virtual monopoly of the power business in Montana and reaches into Idaho. It claims water-power sites in operation and under construction to the extent of 272,000 horsepower and undeveloped sites in reserve with capacity of 162,000 horsepower. The excellent location of its plants is evidenced by the sale with profit of large blocks of power under low load factor at half a cent per kilowatt-hour. Much of this power is delivered over long transmission lines, the company having nearly 2000 miles

of high tension lines. Well located power sites not controlled by the company are located on Kootenai, Flathead, Clark Fork, Madison, Gallatin, Jefferson, and Big Horn rivers in the general territory that it serves.

4. The San Francisco Bay region in California is supplied mainly by the Pacific Gas and Electric Company, the Great Western Power Company, and the Sierra and San Francisco Power Company. Water power plants with capacity of nearly 300,000 horsepower afford the chief power supply of the region and about three times this amount of power is claimed to be held in undeveloped sites by the companies interested. This power is all developed or to be developed from streams on the western slope of the Sierra Nevada Mountains. In addition, the City of San Francisco is now planning to develop for municipal use in connection with increased water supply from Tuolumne River nearly 160,000 horsepower of continuous primary power. The San Francisco region embraces some of the best developed and undeveloped power sites in the West. The sites are of the high-head type, relatively small quantities of water being used under heads of a thousand feet or more. It is necessary to conserve the water supply by storage because of wide seasonal variations in stream flow and the cost of power development is thus somewhat increased if full utilization of sites is accomplished.

5. The Los Angeles district in California is supplied chiefly by the Southern California Edison Company and the Pacific Light and Power Corporation. These companies have developed over 140,000 horsepower and are in position to develop an additional 200,000 horsepower at relatively low cost, chiefly from streams 150 to 250 miles north of Los Angeles, and draining the western slope of the Sierra Nevada Mountains. In connection with the project for increased water supply the city is now undertaking the development of power sites with an aggregate capacity of about 165,000 continuous horsepower from streams draining the eastern slope of the Sierras in the drainage basin of Owens River, and from drops in the water supply aqueduct. The estimated cost of the proposed city system is $91 per horsepower of plant capacity.

6. The Salt Lake district, including southern Idaho and northern Utah, is supplied with power by the Utah Power and Light Company. The nucleus of this system was developed as a series of small plants. These have been connected by transmission lines and

several large plants have been developed and others are planned. The installed capacity of the water-power plants is about 125,000 horsepower and proposed plants aggregate about the same capacity.

7. The Portland district in Oregon is served by the Portland Railway Light and Power Company and the Northwestern Electric Company, which have installed capacity of about 75,000 horsepower and proposed plants with aggregate capacity somewhat greater.

The foregoing are the principal power systems or districts in the Western States. They are operated as public utilities furnishing electricity for street railways, lighting, and small power service, and in some cases supplying large blocks of power under high load factor at rates less than half a cent per kilowatt hour. All the systems mentioned have a surplus of developed power as well as large reserves in undeveloped power sites held wholly or in part under some form of control. In several cases it would be profitable for the systems to contract for disposal of power at rates sufficiently low to be attractive to one or another of the electrochemical or electro-metallurgical industries whose raw materials or finished products would not be subjected to excessive transportation costs.

UNDEVELOPED POWER SITES

The following discussion of undeveloped power sites in the West is based on reconnaissance or preliminary investigation. The stated costs are therefore subject to material correction. Moreover, the estimates have been taken from various sources and are probably not all on the same basis. Comparison of costs given for the various sites should not be made with the idea of obtaining more than a very general idea of relative costs. The capacities stated are in terms of continuous primary power, with assumed plant efficiency of 80 per cent, no account being taken of secondary power.

The principal use of Western water power in electro-chemistry appears to be in connection with the fixation of atmospheric nitrogen for the production of fertilizers, explosives, cyanides, etc. The principal raw materials needed and not universally available are carbon and lime and for some fertilizers phosphate rock. Waste wood from the lumber industry provides a possible source of carbon that is generally available but is rather expensive. Coking coal of suitable quality is not generally available but must be secured

from a few localities. Canada, Pierce County, Washington, the Trinidad field in Colorado, Raton field in New Mexico, and the Sunnyside field in Utah contain coals that are used for the production of coke on a commercial scale. For use in nitrogen fixation processes, however, pure carbon is desired rather than metallurgical coke. It is possible, and even probable, that suitable sources of carbon in other coal fields can be utilized to advantage. It is likely that transportation of coal to the place of use, followed by production of coke under one of the so-called by-product processes, may prove to be the best solution of the carbon problem.

Limestone is widely distributed through the Western States. Stone of suitable quality for burning is located within a short distance of most power sites, though there is a large lava-covered area in Oregon and Washington where valuable power sites exist only at great distances from limestone.

Phosphate rock, so far as discovered in commercial quantities, is located on Bear River in Utah, Green River in Utah and Wyoming, the headquarters of Snake River in Wyoming and Idaho, and on the headquarters of Clark Fork in Montana. All these streams have considerable value for power, though none of the especially valuable power sites hereinafter described are located on them. It is by no means unlikely that the production of concentrated nitrogen-phosphate fertilizers in the phosphate field may prove profitable, particularly in connection with combined irrigation and power storage sites on Green and Snake rivers.

1. Flathead River, Montana. From 1.5 to 5 miles below the outlet of Flathead Lake, Montana, Flathead River has a fall of 180 feet. The flow from the lake can be completely controlled by regulation through a range of stage of 18 feet either by dam or by a tunnel outlet, or both, and the stream would then have a minimum flow of 10,000 second-feet. A combined storage and diversion dam at a favorable site 1.5 miles below the lake outlet, and a canal 2.5 miles in length would make available a net head of 167 feet and provide for the development of 150,000 horsepower. Engineers of the Geological Survey have estimated the cost of dam, canal, power house, and equipment at $3,000,000. The situation would be somewhat complicated by the necessity of securing flowage rights of considerable area on the margin of the lake, but practically all other lands are public. The cost of flowage rights is unknown but may be materially lessened if storage is developed so far as

possible by lowering the natural outlet of the lake. A part of the cost may possibly be borne by power sites on Flathead River and Clark Fork below the site under consideration. If $1,500,000 be allowed for flowage rights the total cost of developing the site of 150,000 continuous horsepower capacity would be $5,100,000, or about $34 per horsepower. Good quality limestone is quarried and burned for lime both north and south of the site within a distance of 50 miles, and unworked limestones are available nearby. The Northern Pacific Railway passes about 25 miles to the south of the power site and the Great Northern and Milwaukee lines are not far distant. Coke would probably have to be obtained from Utah or Canadian fields. Phosphate rock of good quality is within 150 miles on the Northern Pacific.

2. Clark Fork, Washington. Development is proposed at Z Canyon on Clark Fork in northeastern Washington, a few miles south of the international boundary. At this location the river flows through a gorge 80 feet wide at normal water level and 160 feet wide at an elevation 200 feet above the river surface. An excellent site for an arched masonry dam is therefore available though the depth to solid rock foundation is sufficient to make construction somewhat difficult and expensive. Just below the dam site the river makes a sharp bend across which the water will be carried in a tunnel about 1000 feet long to the power house. The effective head will be approximately 260 feet and the dependable minimum flow is slightly less than 8000 second-feet. About 185,000 horsepower can be developed during the low-water period and an additional 150,000 could be made available for 6 months. Full regulation by storage at Flathead Lake, Priest Lake and Lake Pend Oreille would make possible the development of at least 400,000 horsepower through the year. Storage at Flathead Lake alone, as previously discussed in connection with the power site on Flathead River, would add about 125,000 horsepower to the possible Z Canyon development. Estimates of cost have been made by several engineers and these are are reported to indicate a total cost of $7,000,000 without storage, or about $38 per horsepower available during the low-water period.

Good quality limestone is available at the dam site and is quarried on a commercial scale nearby. Coke would have to be secured from the Utah, Washington, or Canadian fields, probably most cheaply from the last named. Lines of the Great Northern

and Chicago, Milwaukee and St. Paul railroads have been constructed within 15 miles of the power site.

3. Salmon River, Idaho. About 70 miles above its mouth Salmon River flows within 8 miles of Snake River and at an elevation of more than 400 feet above it. Several good sites for dams 200 feet or more in height have been found along this stretch of Salmon River and if a dam 150 feet high were built the entire flow of Salmon River could be diverted into the Snake through a tunnel about 7 miles long, piercing the ridge between the two streams. The power house would be constructed on the east bank of Snake River, where a net effective head of about 550 feet would be available. The minimum dependable flow of Salmon River is 4000 second-feet so that 200,000 horsepower could be developed throughout the year. If the tunnel were constructed with a capacity of 6000 second-feet an additional 100,000 horsepower could be developed from April to October, inclusive. John H. Lewis, State Engineer of Oregon, estimates the cost of developing this site at $96 per horsepower for a 200,000-horsepower plant, or $84.50 per horsepower for a 300,000-horsepower plant. Other estimates of cost are considerably less, but so far as known no detailed plans for development have ever been worked out.

There are numerous deposits of good quality limestone on Snake River in this region. Commercial workings of marble and limestone have been developed within 50 miles down stream. Coke would have to be secured from Washington, Canadian, or Utah fields. Somewhat precarious barge transportation down stream to the readily navigable portion of Snake River is a possibility, but would probably not be desirable. A branch of the Oregon Short Line extends down Snake River to within about 50 miles of the site and is projected past the proposed power-house location to Lewiston.

4. Snake River, Idaho-Oregon. Snake River between northeastern Oregon and central Idaho flows in a canyon with comparatively steep gradient. State Engineer Lewis of Oregon is responsible for the following data regarding three alternative sites on this portion of the stream. The first two are below the mouth of Salmon River where the minimum flow of Snake River is about 10,000 second-feet and the flow for 11 months of an average year is about 15,000 second-feet. The third site is above the mouth of Salmon River and would have the same dependable flow as the

other two only in case the Salmon-Snake project discussed above were constructed. In each case development would be of the low-head type, involving the construction of a dam 200 to 220 feet in height, capacities of 200,000 to 300,000 horsepower being indicated on the assumption of the same available water supply for all three sites. A 200,000-horsepower plant at the Coon Hollow site, 45 miles above Lewiston, is estimated to cost $76 per horsepower. A plant of the same capacity at the Cherry Creek site, 51 miles above Lewiston, is estimated to cost $68 per horsepower. At the Mountain Sheep site, 55 miles above Lewiston, the estimated cost of a 200,000-horsepower plant is $70 per horsepower. Quarries of good quality limestone and marble are worked within a few miles of the sites. Lewiston, Idaho, is the nearest railroad point.

5. Skagit River, Washington. In Whatcom County, Washington, between 15 and 30 miles above the town of Marblemount, Skagit River flows through a narrow box canyon, in the lower part of which the fall is particularly rapid. Development of this site by construction of a diversion dam and a conduit, largely in tunnel, about 5 or 6 miles in length, has been proposed. A net head of 490 feet would thus be made available and about 55,000 horsepower could be generated with the minimum dependable flow of 1250 second-feet. About 50,000 additional horsepower could, however, be developed from April to September. If known storage sites were utilized a continuous flow of 2500 second-feet could be maintained and 100,000 horsepower of electric energy could be generated throughout the year. No estimates of cost for the development of this site are available, but conditions are favorable for construction at materially less than $100 per horsepower.

The nearest point where limestone is quarried for the burning of lime is about 50 miles northwest of the site, but good quality limestones are present in the basin of Skagit River comparatively close to the proposed power plant. Coke from Canadian or Washington fields would be most cheaply available. A branch line of the Great Northern Railroad extends up Skagit River to within a few miles of Marblemount.

6. Pitt River, California. Development of power by means of a diversion dam in Pitt River and a tunnel between 7 and 8 miles long across what is known as the Big Bend in Shasta County, California, is one of the many commendable projects in that State. About 1100 feet of fall would be made available and the depend-

able flow of Pitt River at the point of diversion is about 2500 second-feet. Approximately 250,000 continuous horsepower could therefore be developed. A large amount of secondary power could be made available for a part of the year. Press reports of various projects indicate a probable construction cost of $30 to $50 per horsepower for plants of 120,000 to 250,000 horsepower capacity.

The nearest limestone quarries are perhaps 50 miles from the proposed power plant, the Southern Pacific Railroad being at the same distance. Probably Utah coke could be obtained at least cost.

7. Colorado River, Arizona. Preliminary surveys have been made for seven power sites on Colorado River in northwestern Arizona, with aggregate estimated capacity of over 200,000 horsepower. The river is in canyon for many miles and an enormous amount of power could be developed in plants of the low-head type located at favorable dam sites. The flow of Colorado River will probably be regulated by storage to give a fairly uniform flow to meet the requirements of irrigation projects in southern Arizona and California, thus greatly enhancing the continuous horsepower capacity of available power sites. The present dependable flow of the river is sufficient to develop about 4400 horsepower per foot of fall. At the lower end of Boulder Canyon, about 15 miles below the mouth of Virgin River, a dam 125 feet high and 435 feet in crest length will make available an effective head of about 110 feet and provide for the generation of 48,000 horsepower. An additional 50,000 horsepower could be developed from February to October. Under present conditions of stream flow the range of stage would be an obstacle of efficient operation. The cost of this project is estimated at about $1,000,000 by the promoters. The project would be an attractive one, even if the cost proved to be three times this amount.

Limestone is available practically at the site and is quarried and burned at a point not more than 25 miles distant. Utah and New Mexico coke would be most cheaply available. The San Pedro, Los Angeles, and Salt Lake Railroad passes within 20 miles of the dam site.

8. Green River, Utah. Mention should probably be made of the Flaming Gorge reservoir site on Green River in northeastern Utah. By raising the water level 215 feet storage capacity of over 3,000,000 acre-feet could be developed, about 1,200,000 acre-feet

of which would equalize the flow of the river to a minimum flow of 2700 second-feet. The dam, in connection with a 3-mile tunnel, would make available a head of 290 feet and provide for the development of somewhat more than 70,000 horsepower. Equalization of flow at the Flaming Gorge site would be of material value to irrigation interests on lower Colorado River, and if the cost of storage were borne by them the power development would be a very cheap one. Limestone of good quality is found in the region and phosphate rock outcrops at the dam site. The Utah fields of coking coal are close by, though there is no direct railroad route to them. The backwater of the dam would extend to within a few miles of Greenriver, Wyoming, on the Union Pacific, the nearest railroad station.

9. Deschutes River, Oregon. The results of an investigation of the power possibilities of Deschutes River, Oregon, are published in Water Supply Paper No. 344 of the U. S. Geological Survey. It appears to be feasible to develop the lower 120 miles of the stream by a series of dams and power plants with aggregate capacity of 500,000 horsepower. At one site about 65,000 horsepower can be developed at a cost estimated by the State Engineer of Oregon to be $61 per horsepower.

Railroad facilities are excellent in this region, but no limestone is available except at considerable distances.

10. Columbia River, Oregon and Washington. No list of important Western power sites would be complete without mention of the site near The Dalles, Oregon, recently investigated jointly by the State and the Department of the Interior. Here about 480,000 continuous horsepower can be developed and installation up to more than a million horsepower would be warranted if market for the power could be found. There are material engineering difficulties to be overcome in constructing the proposed dam across Columbia River, and the principal cost of development would have to be borne in such construction at the inception of the enterprise. This, in the absence of an established market for large blocks of power, tends to make the project of doubtful feasibility at the present time. The cost of raw materials makes the use of power for most electro-chemical and electro-metallurgical industries at this site somewhat questionable. The estimated cost of a project for the development of 480,000 continuous horsepower and an additional 320,000 horsepower available for eight or more

months of the year is $50,000,000. Power in large block under high load factor could probably be sold for $10 or less per horsepower year.

The fact that the Western States afford excellent power sites that can be developed at relatively low cost can not be controverted. Reasonably rapid development of these power resources by public utility corporations is taking place and construction is in general made well in advance of the demands for power. Extensive transmission systems are being developed to cover entire States, some systems being 500 miles or more in extreme length. Hydroelectric power is being used extensively in mining and there appears to be a bright outlook for electric smelting of ores in some localities. The electro-chemical industries are practically unknown in this field. The raw materials for some processes are available in various localities. Freight rates are high, however, and the principal market for products is distant a thousand miles or more. Under these conditions selection of water-power sites for development must be made with great care if commercial success is to be attained.

DISCUSSION.

A MEMBER: Mr. Chairman, there is one thing that struck me in the reading of this paper, and that is that the discussion of water powers available in the United States is limited almost entirely to the West; in fact, limited completely to the West, when, as a matter of fact, there are water powers available of quite some magnitude in the four States of Georgia, Alabama, Tennessee and North Carolina, where some projects are under way and some development has been made toward utilizing these for electro-chemical purposes, notably in Tennessee.

Mr. LIDBURY: Mr. Chairman and gentlemen, I must apologize for this belated discussion. I was unable to be present earlier in the meeting and came down to-night in the hope that I might hear the paper of Mr. Stabler. Through the kindness of the Secretary I have been able to peruse this paper and permission has been granted me to discuss it.

I think there is no more important subject for the consideration not only of chemists and chemical engineers, but also of engineers in general, than the question of the utilization of the water

powers of the United States. I think this Institute is to be congratulated upon the presentation of this very able and exhaustive paper. Mr. Stabler has given us in this paper a survey of a number of water powers developed and undeveloped in the West, and has also given us by way of introduction some very well thought out expressions in regard to the utilization in general of the water powers of the United States.

I shall first briefly recall one or two of the things he has mentioned. He points out in the paper that the Federal Government is the sovereign proprietor of three or four million acres of land west of the Mississippi, and that perhaps half of the ultimate water power in the United States will involve the occupation of what are now public lands; and, in the course of the paper, he proceeds to consider what the possible utilization of these water powers under government control is. The question of water powers immediately under government control I shall come back to later, but I think that this is at present a minor matter in comparison with the general governmental attitude towards the development of water power.

Another point which is emphasized in this paper is as follows: Cheap power is desirable for electro-chemical and electro-metallurgical processes, though not necessary for all. This is another point that I would like to come back to later. I am merely taking one or two things that Mr. Stabler has emphasized, as I want to put them in a somewhat different light later on.

Then Mr. Stabler proceeds to point out that too much should not be expected of the West, wonderful as its water power resources are, but that there should be a steady development *to supply the markets of the West,* and that with reference to *some* industries, there is reason to believe it may be possible to market finished products in competition with those developed with the better Eastern water powers; that this is increasingly true as the water power in the East becomes exhausted.

In connection with that, I would like you to observe the extreme conservatism of Mr. Stabler in stating the case, and I want to show you that his conservatism is by no means overdone. He carried it through his discussion of the individual water powers to which he alludes. In the first instance, that of the Puget Sound Traction Power and Light Company with some 75,000 horsepower developed in a possible ultimate development of 175,000.

He states this undeveloped power merely *awaits a suitable market.* Now, taking some of these points in order.

Mr. Stabler has very clearly pointed out the commercial factors in the development of a water power; he has gone into the cost of steam power under various conditions; he has gone into the cost of water power under various conditions, and he has very clearly shown what is perhaps not always grasped, that the cost of power in a hydroelectric development depends almost entirely upon the capital cost of that development. Perhaps in stating that the cost of a horsepower per year is something like one-seventh of the capital cost of the construction cost of the horsepower in that particular develpment he is getting about as near to giving a general figure as can be done. Of course, such relations vary vastly in different cases under different conditions, but he has come pretty nearly striking a fair average in that respect. He has also pointed out the factors upon which the ultimate commercial success of any water power development depend; but I fear that he has not emphasized the relative importance of the different factors, but it seems to me rather, feeling that there is no immediate future of any considerable magnitude for the Western water powers, he has rather avoided taking the bull by the horns and saying that this is so, and why.

Now, the one essential thing that has to be borne in mind in considering the value of a water power is its location. It is perfectly true that you may have water power, say in Idaho, that you can develop and sell, let us say as low as ten or fifteen dollars per h.p.-year, but that water power development is not going to be a commercial success unless you can get customers who can buy that power and use it and *market their products profitably.*

There seems to have been among engineers, as distinguished from chemists and chemical engineers, the general habit of looking until very recently at water powers as having their ultimate market in the kind of thing for which water powers in general have been largely used in the past, namely, municipal loads, traction, lighting and similar power loads in the vicinity. Now, this matter was very thoroughly discussed at a meeting of the Institute of Electrical Engineers in Washington at the end of April, and the general conclusion that one came to from hearing the very able papers presented on the subject was that for that class of load, at all events, where the load factor is usually poor and

where, in most locations where population density is fairly heavy, steam power can to-day be produced at a much lower cost than was the case a few years ago,—the general conclusion, I say, was that for that kind of service water power probably had no future. It is true that conditions would not be as stated in those vicinities where fuel is dear and where at the same time water power can be developed at a comparatively low per horsepower cost, but in the majority of cases for any given locality in the country it is very difficult to make out a case on any consumptive basis such as that for the economical successful development of water power; and the consequence is that there is coming more and more the tendency to look to electro-chemical outlets as the proper consumptive outlets for the unutilized water powers of the country. This is for two reasons: first of all, because, though the ordinary municipal and similar loads have a remarkably poor load factor, the electro-chemical loads have a remarkably good one, and in a commercial venture where so much depends upon the amount of capital involved and the fixed charges and so little depends upon the actual running cost, the load factor is the determining item; and secondly, because electro-chemical products form a mode by which that energy can be transported from place to place, whereas the transportation of electrical energy is so limited as to distance; and it is therefore desirable, in view of the fact that this point of view has cropped out so strongly (and you see it cropping out on almost every page of Mr. Stabler's paper), to look critically at the conditions which justify the utilization of hydroelectro power for electro-chemical purposes.

It is true, as Mr. Stabler points out, that the cost of water power it only one item of the requirements of electro-chemical processes. There is the cost of labor, the cost of transportation, the proximity of raw materials, and so forth; but when one begins to analyze the proposition in detail and takes specific instances, one becomes convinced that of all the factors outside of cost of the electric unit, the one of transportation to market is the most vital one in nearly all cases.

Now, Mr. Stabler has limited himself to a consideration of the Western water powers, and he has seemed to take the point of view that to a large extent the development of those powers depends upon their being tied in with electro-chemical industries in the West. What chance is there of that?

There are electro-chemical industries that have never succeeded in obtaining a foothold in this country because of their requirement as a fundamental condition for their existence of a power supply at a price much lower than anything that is obtainable in this country. A very good instance, which is familiar to all of you, is that of the fixation of nitrogen. On the other hand, most electro-chemical industries which have established themselves in this country have established themselves in localities which are characterized by two factors: first of all, by the existence there of power at reasonable though not really cheap rates; and secondly, by proximity to the markets which those industries have to serve. It is very easy for any one familiar with the general subject to take, for instance, a list of the products which are manufactured at our principal electro-chemical center, Niagara Falls, to sum up the total power cost and to compare it with the transportation cost of the finished product, and, bearing in mind that the conditions existing in the West in regard to raw materials are in no case better, and in many cases a good deal worse, than they are in the East, he will come across some very interesting and curious results. It amounts to this: Taking those industries, I found in comprehensive calculations carried out for this purpose that there was perhaps one which could afford to transport itself to Western water powers, provided that the powers were remarkably cheap. On the other hand, at the other end of the schedule, there was another industry that could under any circumstances afford to transport itself to Western water power sites to a limited degree, to a degree which would enable it to serve those markets which existed in the West. In the one case you have the extreme where transportation of the finished product is relatively light compared with the cost of power; and, in the other case, you have the extreme where transportation is extremely heavy compared with the cost of power. But, leaving out these two extreme cases, it is a curious thing that practically all the industries at present located in Niagara Falls would require power to be furnished them in the West at prices varying from thirty to sixty dollars per horsepower year less than that furnished in the East in order to enable them to exist in competition with their present locations. In other words, they would not only have to have the power handed to them free, but with a substantial bonus.

Why is this? It is because these products of the electro-

chemical industry are not articles which go into a state of ultimate consumption immediately, but because they furnish extremely important fundamental materials for our basic industries, which are as a general thing grouped in the East; and, as a consequence, leaving out one or two special cases, it will be found upon a careful examination that the future of the general electro-chemical industries in this country must be in the East or there will be no future at all.

I said leaving out one or two special instances. There are, for instance, things like the refining of copper which is not dependent on the cost of power, because cost of power does not cut any figure compared with the transportation costs and the interest tied up in copper and that sort of thing. On the other hand, there are big fertilizer propositions in the air, but which never seem to materialize, which, again, are largely a transportation problem, so that if we are going to have a big electro-chemical fertilizer plant it must be near where the fertilizer is used. The present use is in the East and South; the South is the large consumer. There is no fertilizer, speaking broadly, used in the West in the great grain fields, and how long that condition will exist nobody knows, but the best calculations on the matter I have seen seem to show that until the price of fertilizer, particularly nitrogenous fertilizer, gets down to half its present cost there is no possible chance for the grain farmer to use it to his commercial advantage, because the more he uses it the more it increases his cost, and although it may be a very desirable thing to grow two stalks of wheat where one grew before, he won't grow them unless he gets more for growing two than for growing one. Consequently, we are forced to the conclusion that except for the consumption of articles which can be used in the West, and again, in the rather remote contingency of the West being able to supply any markets that may be developed in the future in the far East, the electro-chemical industries will in so far as they continue to exist in this country largely remain in the East.

Now, I referred to the fact that this paper was the result of government inquiries on what the best thing the government could do for water powers was. I will tell you the best thing the government can do in regard to the water power situation. It is, forget all about these water powers that happen to exist on government property in remote districts and remove instead all the

present hindrances to the development of the extremely large unutilized water powers that we have in the East. We meet this in two ways. There is, firstly, a condition at present which has absolutely prevented the development of any considerable water power for the last seven years. This is the fact that the Federal Government regards almost every stream as a navigable stream, and that the sanction of Congress by a special bill has to be obtained in order to develop power on any navigable stream. We meet in this respect a remarkable popular prejudice against the development of water power. Water power is supposed to be a gold mine; it is supposed to be something that the infernal capitalist is trying to rob the people of, and, consequently, it has been impossible to get any reasonable bill through Congress in the last six or seven years. As a matter of fact, of course, the situation isn't anything of the kind. Most of the water powers that have been developed haven't paid and people have got a little careful of putting their money into that kind of proposition. Not only is that so, but the general tendency to restriction and to the insertion of onerous conditions in regard to governmental interference has frightened people off, because no one has ever known what was going to be clapped on the already sufficiently onerous conditions under which water power suffers to-day. And, secondly, we find that in the most remarkable instance of a successful development of water power we have in this country—Niagara Falls—the further development of the water power there has come to a sudden halt, again on account of governmental interference, governmental interference based upon popular prejudice fostered by the agitation of charlatans.

The Niagara situation is becoming a matter of extreme seriousness to the industries of the country at large, and I should like you to bear with me for a minute or two longer while I tell you exactly what the situation is, because I do not think that even in technical circles it is quite realized. There is a matter of some two hundred thousand horsepower being developed on the American side of the Niagara River; on the Canadian side the development runs, I think, about 350,000 horsepower. When the restriction on further development was put into effect it didn't so much affect the Canadian side as it did the American side, for the reason that the market on the American side was already abundant, that on the Canadian side was not much in evidence, and, as a con-

sequence, when the restriction on importation of power from Canada was removed with the lapse of the Burton Act, the amount of power imported into the United States from Canada in the Niagara district increased very considerably until we had a condition where we were generating ourselves on this side about 200,000 horsepower and importing perhaps as much as 175,000 horsepower. There has always been since the restrictive measures went into effect a condition of relative stringency on the American side. This has resulted in an absence of competition, in stiffening of prices, and an increasing difficulty in obtaining power. At the present moment the condition is one of absolute famine. Not only can no new customer go into the Niagara district on the American side and obtain power, but those concerns who have been there for a number of years are finding it extremely difficult to get enough power to fill their needs. On top of this, we are now advised that the Canadian Government, having practically reached the limit of the development allowed under the treaty which governs the quantity of water to be diverted by either side, has reduced, and proposes in the future to continue to reduce, the exportation of power from Canada into the United States by amounts of approximately 15,000 kilowatts per year, so that we shall suffer a gradual diminution of the power which is at present being used, and, so far as we can see, in seven or eight years' time there will be no power imported and the operation of companies now engaged on the American side in the production of electro-chemical products will be greatly curtailed unless some assistance which is not in sight appears, for their power will be reduced by pretty nearly half of what it is at present.

I don't think in this audience I need expatiate upon the importance of the electro-chemical products to the general industries of the country. If there are any of you who are not familiar with the fundamental importance of those to every single industry there is, to everything that affects you from the time you get up in the morning until you go to bed, then I might refer you to a symposium that was given in Washington at the last meeting of the American Electro-chemical Society, at which several papers were presented. It has been particularly interesting to me to-night to notice that one of the things that the chemical industry has had to fall back on in these strenuous times has been evaporating and other plants of high silicon metal, which would be impossible with-

out the electric furnace, without the ferro silicon and silicon produced at Niagara Falls; and I think you even went so far as to say that these things had to be ground—Niagara Falls again. You cannot do grinding as it is done to-day without the use of the modern abrasive, the artificial abrasive; the old grinding materials have no place in modern industry. However, it would take me all night to go over the importance of Niagara products to industry. I am only going to insist that, although we have heard a lot of things about dyes and about not being able to get colors for goods, we would have been up against a much more serious proposition, with many of the greatest industries scarcely able to operate at all, if those electro-chemical industries which I am talking about had not been in this country; and if they are going to be restricted, if their operations are going to be curtailed by the absolutely unnecessary limitation of the legitimate and reasonable use of our natural resources in the way of properly located and cheaply developed powers, the next time world commerce is disturbed we shall find half these industries out of the country. We can see this by what has already taken place. Since these restrictions went into effect we find there has been a general tendency of electro-chemical industries centered around Niagara Falls to emigrate. Where to? Not to the West; not, as a general thing, to other powers of this country, but to other countries. I could give you five or six instances of Niagara Falls companies whose major operations as regards their basic electro-chemical products are not in Niagara Falls, New York, but in Canada, or in France, or in Norway. This migration is a process that we see going on before us every day, and it will continue to go in the future.

We hear of numerous developments such as the proposed Duke Fertilizer Plant, which is going to involve a matter of some 250,000 or 500,000 horsepower, to produce soluble phosphates and nitric acid. Where is that going to be located? Not in the States, but on the Saguenay River in Canada. Where is the market going to be? We are the market for this and the other wandering industries I have mentioned—in the States.

You all know what the results of depending for fundamental materials on foreign nations has been in the last two years. What we have got to meet in this water power matter is nothing more or less than this: Is the government going to allow, is Congress going to permit the water power resources of this country, par-

ticularly those at a reasonable distance from our industrial centers, to be used; or is it by putting obstacles in the way of the use of those water powers, going to drive these fundamentally important industries away to foreign countries, and get a condition which in time of war—not necessarily our wars, but somebody else's wars, perhaps—may put us very seriously in the hole for things that we absolutely need?

Those of you who are aware of the importance, for instance, of the ferro alloys in the steel industry; those of you who know the extent to which the steel industry is dependent for products of that kind to-day, will realize what a mess this country would have been in in the last twelve months if it had not been possible to pretty nearly, by throwing overboard the production of some electro-chemical products which are not so important, take care of an article which has been hitherto imported to an extent of at least fifty per cent of our requirements.

That is the thought that I would like you to bear in mind whenever the question of water power comes up. And it seems to me that organizations like this have their responsibility in the matter. It seems to me that they should—each member of them should—do what they can in the way of general education in the matter, because there is going to be no let-up, there is going to be no improvement until the average man, the layman, the person who does not understand technicalities, understands exactly what the development of water powers in regard to electro-chemical products means to him individually and means to the prosperity of the country; and there is no way by which that average man can be got at other than by the efforts of technical men and those who know.

The Institute of Electrical Engineers, I am glad to say, has taken some steps to get publicity in regard to the importance of the use of water powers. I am afraid, however, the electrical engineers do not understand this phase of the matter as well as they might. I am glad to say that the American Electro-Chemical Society is taking very vigorous steps to obtain publicity in the matter, and I think it is a matter that this Institute might consider,—whether there is not some duty in this respect that it owes to itself, to the chemical industries, to the industries of the country in general, and to the community, in the way of education.

(Applause.)

CHAIRMAN: The Chair wishes to congratulate Mr. Lidbury upon his highly interesting discussion of Mr. Stabler's paper, or, rather, upon the presentation of his own admirable paper; and the members are to be congratulated upon having had the privilege of having this very timely and interesting and important subject laid before them in this convincing and able manner of Mr. Lidbury's. I think the suggestion he made in reference to the Institute is one which deserves the attention of the Institute, and the Chair would be glad at any time to entertain any motion from the floor with reference to the suggestion made by Mr. Lidbury. The subject itself is such a broad one, such a big one, that I do not know whether our time will permit of a discussion which at best would be inadequate to do the subject justice; nevertheless, we shall make a start and the subject is now open for discussion.

Mr. MARSH: I should like to ask Mr. Lidbury a question, and that is, whether the power developed at Keokuk, Iowa, on the Mississippi River, is available, and whether electro-chemical industries could utilize that power, and, if he can, to give us some idea as to how much is available?

Mr. LIDBURY: My understanding is that the development at present is of about one hundred thousand horsepower, of which there are perhaps twenty or thirty thousand still available. It is supposed to be possible to develop another one hundred thousand horsepower there, but whether that is strictly the case I do not know. Of course, the consideration of the availability of any particular power is primarily dependent upon the transportation factor, which I tried to emphasize; and while I think there is no question but that certain industries might locate at Keokuk, it has to be remembered that it is at a considerably greater distance from the industrial center than, say, Niagara Falls, and, as I pointed out in our meeting at the Electro-Chemical meeting in Washington, it may be in a great number of cases, with ordinary conditions of water transportation, that Norway, with its cheap powers, is a good deal nearer to the consumptive centers of this country than even Keokuk is.

Mr. MASON: I should like to ask a question with reference to the amount of power that the United States is taking from Niagara. Is it all that it is entitled to take under the treaty?

Mr. LIDBURY: Not under the treaty, no. There are 4400 cubic

feet of water per second still available under the treaty, the provisions of which are more liberal than those of the Burton Act, but since the Burton Act expired the War Department has asserted the right of saying how much water should be taken and until Congress has taken the necessary steps they will keep this 4400 cubic feet unused and won't let it go. Buffalo has been up against it; they are building a steam power station, and until that is built they will be up against it for power. Buffalo, I understand, has been able to get a certain amount of this 4400 feet let loose for the time being until they can get the station built. One of the power companies at Niagara Falls has machinery installed which it cannot operate because the water supply is so curtailed.

Mr. MASON: Has Canada the full amount of power to which she is entitled under the treaty?

Mr. LIDBURY: Canada has still about 6000 cubic feet per second available which she is taking steps to develop immediately.

Mr. MARSH: Mr. Lidbury's discussion has brought out, I think, and is now bringing out a thorough searching out in the Eastern districts of the available unused water power, whether surplus or undeveloped. It has been my privilege to make an examination of several of these conditions, and it is coming to be of marked interest—the available surplus water power of present developments, possible extension of present developments and the extent that various of these products made at Niagara Falls will very likely be made in various parts of the Eastern district within the market conditions of that locality. From my study, it seems that every water power in the eastern part of this country must be largely subject to its local conditions for a market and that surplus power must be used to the best advantage. An interesting phase of this question is brought out very plainly down in Boston. We have all known that water power usually is rather uncertain, but it has so happened within the last year or two that they have had plenty of water and have taken advantage of it by making a very large profit to the companies engaged in it, but especially the fact that there is available for service steam power equipment, which is ready to be thrown into use at any time when this water power surplus is not available, makes it possible to extend vastly the amount of power which will be used by electro-chemical processes in the Eastern market centers of the United States. For that very reason it is timely that Niagara

Falls is bringing out, causing a searching out, of the unused water power surplus in the eastern parts of the country. So the present disadvantage has some value, although I thoroughly believe that we cannot disregard the fact that the government is cutting its own throat by limiting the amount of power that can be developed at Niagara Falls, at the same time to our own disadvantage.

It has been my contention that for a long time the Western water powers are seriously limited on account of the disadvantage of great transportation. There is a serious study to be made of the question of our water powers and the relation of our government to their development.

CHAIRMAN: Is there any further discussion? Will we hear anything further from you, Mr. Lidbury?

Mr. LIDBURY: The only thing I would like to know is if Mr. Marsh does not agree with me that under present conditions it is just about as difficult to start to work to develop other Eastern water powers as to get any more development at Niagara Falls,—I mean with the present state of legislation?

Mr. MARSH: There are some parts that are not restricted. Of course, the larger amounts of power are restricted. That does not necessarily mean big streams; but there are small amounts where local conditions, local markets can be supplied, so that each water power should be studied individually.

Mr. LIDBURY: That is perfectly true, but you wouldn't state that under present conditions the possibility of developing other powers in the eastern part of the country will afford any serious relief from the situation that obtains to-day?

CHAIRMAN: Would you locate any on those small water powers?

Mr. MARSH: Oh, yes. They are seriously being considered.

Mr. LIDBURY: Of course, things are being done to-day that people will be sorry for doing in two years' time.

Mr. MARSH: Of course the development of water power can bring other power down to the point of reason, and the water power competes to a point where they can afford to sell that power for a reasonable rate for the electro-chemical and metallurgical industry.

CHAIRMAN: I think I voice the sentiment of the gentlemen present when I say this has been a very profitable and interesting discussion. We shall now proceed with the balance of the pro-

gramme. May I inquire if Dr. Worden is present? He seems to have failed to turn up, much to our regret.

Mr. STABLER: Mr. Lidbury appears to have received the impression that I can see no future for Western water powers. On the contrary, I believe there is a very bright future for them. Hydroelectric development in the West has proceeded and is still proceeding with remarkable rapidity. In many lines it is far in advance of the East and will remain so. Certain types of market, such as irrigation pumping, and mine operation, that are almost unknown in the East, are highly developed in the West, and will be extended almost indefinitely. The diversity of load in the great power systems of the West is such that high load factors are the rule, and they are becoming more favorable from year to year. The water-power resources are enormous and it will, of course, be many years before they are fully utilized, but the outlook for the industry, developed for the most part along public utility lines, is decidedly favorable. In the field of electro-chemistry, however, the Western outlook is restricted. Electro-chemistry, generally speaking, is dependent on great industrial and manufacturing centers for a market, and it is not likely to advance in the West except as the industries of which it is a feeder are developed there. Agriculture, with its ultimate demand for fertilizers, appears to afford the best basal market for electro-chemical products in the West, and, as suggested by Mr. Lidbury, this market is not likely to be very great for some years to come, until the soil begins to show signs of exhaustion.

It will be seen, therefore, that I am by no means prepared to accept Mr. Lidbury's suggestion that the best thing the Government can do is to forget about the water powers on the public domain. As a matter of historical fact, they were forgotten for some years after long-distance transmission became practicable, with the result that when they were remembered they had, to a great extent, been alienated under the guise of homesteads, timber claims, mining claims, lieu selections, and so forth. I offer as a counter-suggestion that the water powers on public lands should be wisely administered under a well-considered water-power law.

Development of water power, reasonable, orderly, economic development in the public interest is highly desirable. Development "of the extremely large unutilized water powers that we have in the East" is particularly desirable in the interests of electro-

chemistry. I agree with Mr. Lidbury that water power is not always a dividend producer, far less so than has been popularly supposed. Many large power developments have proved to be rank failures, some from engineering mistakes, others from ill-considered promotion, and still others from financial legerdemain. An interesting view of these failures is reached if an attempt is made to locate those who have actually lost money by them. So far as my observation goes, it is in most cases the relatively small investor who loses, and the indications are usually strong that at some stage of the proceedings he has been the victim of the gentle art of freeze-out.

Mr. Lidbury calls attention to the situation at Niagara Falls, but tells only a part of the story, laying considerable stress on what he terms "governmental interference based upon popular prejudice fostered by the agitation of charlatans." While this is foreign to the subject of my paper, it is of sufficient interest to warrant brief discussion, though too complicated in detail to receive adequate treatment. As I see it, the interest of the Government, that is, of the people, in water power lies in efficient economic development under such control and regulation that all parties in interest will receive a fair degree of the benefits to be derived. At Niagara there is a special interest of the general public in the scenic beauties, and if people as a whole prefer scenic beauties to water-power development it is unquestionably their privilege to have the scenic beauties unimpaired. Aside from this, we have at Niagara a manufacturing market and a public-utility market. On the United States side of the river manufacturers have been accorded the benefits of cheap power by the generating companies, whereas such privilege has not been voluntarily accorded to the citizens of Buffalo. Not only have rates for electric service in Buffalo been high, but by reason of a chain of corporations ingeniously devised, the State public service commission was powerless to order an adequate reduction, although in 1913 a material reduction in rates was ordered. At present it seems probable that the citizens of Buffalo will be excluded from any possibility of sharing to a greater extent in the benefits of cheap Niagara power, for the public-utility company is building a large steam plant to supply electric service to Buffalo and surrounding towns, and power from steam, under the best conditions, costs about three times as much as water power at Niagara Falls. There is, perhaps, in this situation some ground

for popular prejudice. In this case, too, water power has proved to be a very profitable investment, for one of the New York generating companies reports surplus for dividends of six and a half million dollars in the last ten years, and the other is also a dividend producer. There is a matter for consideration also with regard to the fall utilized. The difference in elevation between Lakes Erie and Ontario is 325 feet. The fall from Lake Erie to the water surface below the falls is 226 feet. From the head of the rapids above the falls to the water surface below the falls there is a fall of 212 feet. One of the large power companies in the United States operates under an effective head of 135 feet and the other under an effective head of 200 feet. In view of these facts is there not some reason to require more complete use of available head before permitting the diversion of additional water?

On the Canadian side the interests of the public are not left wholly in the hands of the allied power corporations. The Ontario Hydroelectric Power Commission contracted for a large block of Niagara power for use mainly by Canadian municipalities. It is chiefly the additional load under this contract, under which the people receive a material benefit, that appears to have scared Mr. Lidbury, for a demand of 50,000 horsepower has recently been made by the commission. Furthermore, there is a very definite policy in Canada that in any additional power development assurance must be given that the municipalities of Canada will have an ample supply of electric energy. From the viewpoint of a mere citizen it appears that a little more of the type of governmental interference exercised in Canada would have a beneficial influence in the United States and would remove some of the popular prejudice that exists. Action by a governmental agency ought not, however, to be necessary in this regard. A far-sighted policy on the part of power companies, giving the people of the communities in which they operate adequate electrical service at the lowest rates compatible with the service rendered would engender a popular prejudice in their favor. Such is the situation under certain large power systems in the West. At Niagara Falls not only does the development that has been made provide for rather inefficient utilization of the resource, but the local populace has been overlooked in the scramble for dividends. There is likely at any power site to be a conflict between the energy requirements of chemical industries and of the general public. In such case, unless there is a fairly

equitable distribution of electro-energy and of the benefits arising from the natural advantage of the site, a "popular prejudice" against the water power and electro-chemical industries is bound to arise and must be reckoned with. So far as legislation is concerned, a strong popular feeling is almost sure to control. Square dealing is better than a paid lobby, and adequate service at reasonable rates affords a more convincing argument than inspired articles in the popular and technical press.

THE TREATMENT OF SEWAGE BY AERATION IN THE PRESENCE OF ACTIVATED SLUDGE.—II.

By EDWARD BARTOW

Read at the Cleveland Meeting, June 16, 1916

Since reporting the results of our work with activated sludge at the San Francisco[1] meeting in August, 1915, experiments have been continued. Experimental work has been carried on in the four reinforced concrete tanks mentioned. The tanks are 3.17 feet square, having an area of 10 square feet and are 8.42 feet deep. These tanks were constructed in the basement of the boiler house of the University of Illinois and are operated under conditions similar to those which would be found in a housed plant. (See Fig. 1.) The sewage is pumped by means of a centrifugal pump to the tanks from the main sewer of the city of Champaign. Compressed air obtained from the University compressed-air supply is reduced from 80 pounds pressure through a special reducing valve to 8 pounds pressure and is further regulated by hand operated valves before passing through meters at each tank. The pressure under which it enters each tank is sufficient only to overcome the pressure of the sewage and the friction of the plates. The bottoms of two tanks (See Fig. 2) are entirely covered with Filtros plates. The bottom of the third has three plates forming the bottom of a central trough with the sides sloping to the plate at an angle of 45°. The bottom of the fourth tank has but one plate with the bottom sloping from all sides to the plate. Outlets for the effluent were placed at 2.5 feet and 5.6 feet above the porous plates and an outlet for the sludge, closed by a quick operating valve, was inserted just above the plates.

Experience in operating the tanks indicated that a lower outlet connected to a floating outlet was desirable. A fixed outlet is objectionable because sludge is at times drawn out with the effluent. In later experiments, a floating outlet made of 2-inch pipe connected together with loose joints was placed in one of the tanks. By this

[1] Transactions 8, 119–131 (1915).

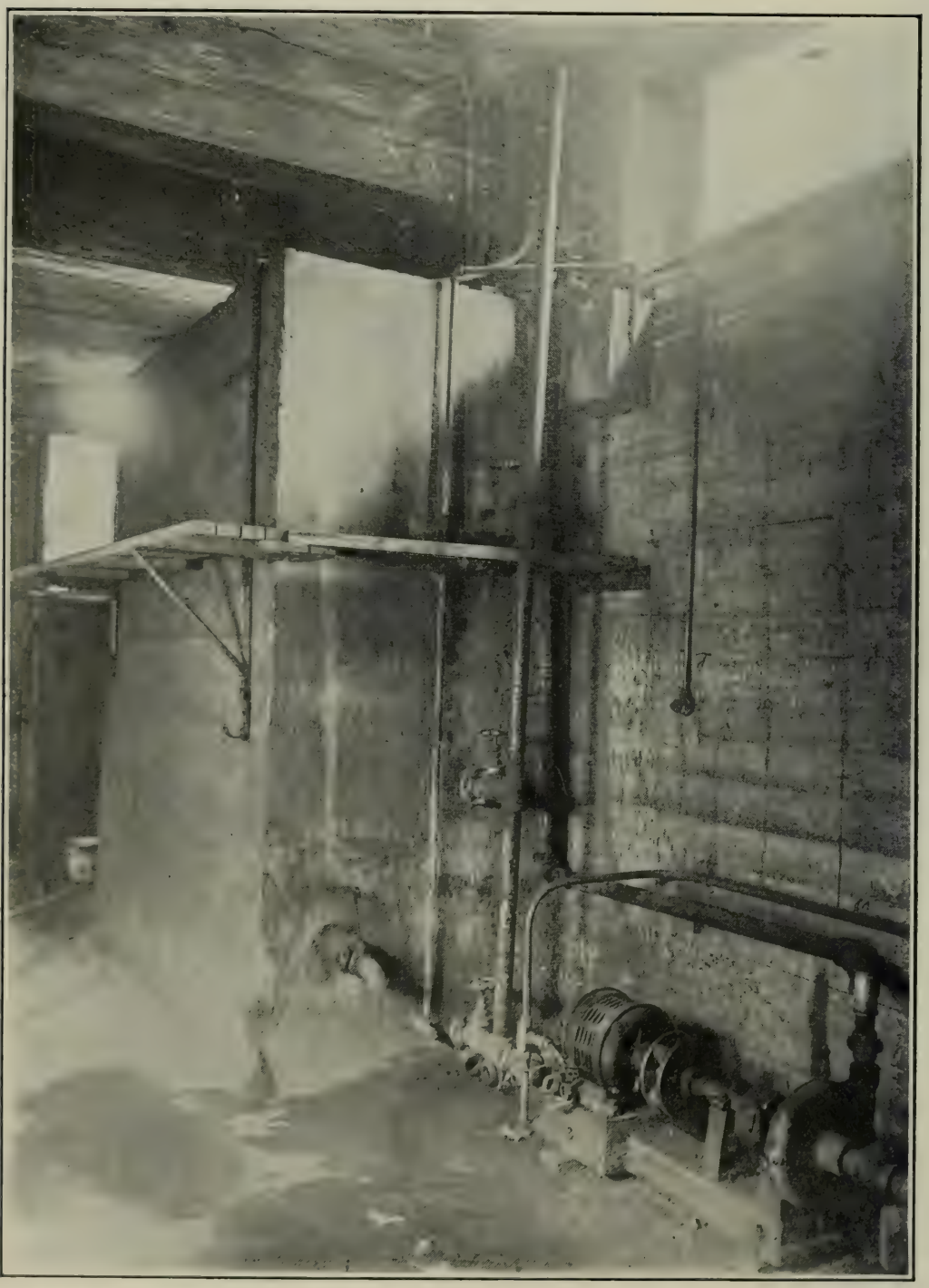

FIG. 1.—Concrete tank used for the treatment of sewage by aeration in the presence of activated sludge.

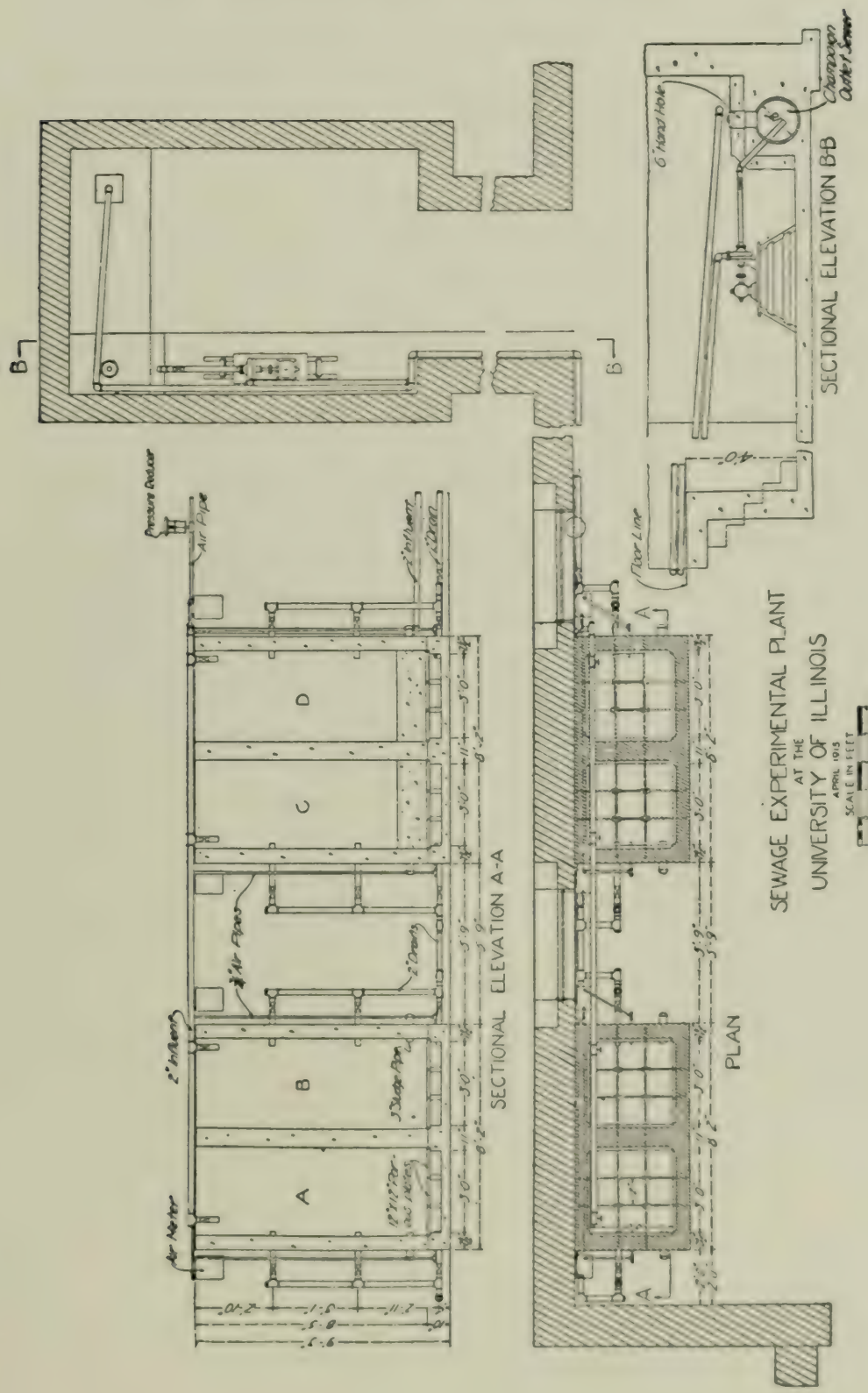
SECTIONAL ELEVATION B-B
SECTIONAL ELEVATION A-A
PLAN
SEWAGE EXPERIMENTAL PLANT
AT THE
UNIVERSITY OF ILLINOIS
APRIL 1913
SCALE IN FEET
A
B
C
D
Air Pipe
Air Meter
Pressure Gauge
Floor Line
6' Manhole
Champaign Outlet Sewer

FIG. 2.

arrangement no sludge was lost and more accurate data were obtained concerning the amount of sludge formed from the sewage.

Building-up of Sludge.—If activated sludge must be built up by complete nitrification of each portion of sewage added, several weeks would be required to put a plant in operation. In order to obtain sludge more quickly, the English investigators[2] have used sludge from sprinkling filters. At Milwaukee[3], Imhoff tank sludge has been aerated until it is aerobic and similar to activated sludge. Sludge from such sources would not always be available, especially at newly installed plants. We have, therefore, attempted to shorten the period of sludge formation. Two tanks were filled with the same kind of sewage. Air was passed into one continually, into the other for 23 hours, after which the sludge was allowed to settle for one-half hour, the supernatant liquid withdrawn and the tank refilled with fresh sewage. This cycle was repeated daily. At the end of ten days, after one hour's settling in Imhoff cones, there was 10 per cent of sludge in the tank which had been changed daily and one per cent in the one which had been aerated continuously. Effluents were equally stable and that from the tank which had been changed daily was clearer.

By further experiment it was found that activated sludge could be built up using a 6-hour cycle, and since considerable purification of the sewage was obtained from the start, no special effort need be made to obtain a suitable activated sludge for starting a new plant, or renewing the sludge after a shut down.

Diffusion Area Required.—Better results were obtained in the tank containing three plates than in the tanks containing one or nine. The inefficiency of the tank containing nine plates may have been due to poor distribution of air, to deposits of sludge accumulated in places where the air was not passing through, or to inefficient mixing of the sewage, sludge, and air. When only one plate was used, it was difficult to force enough air through it for purification without giving too violent ebullition, or possibly the flow of air was too rapid to allow sufficient contact between the sludge, sewage, and air. It is possible that an area between one-third and one-ninth will be most efficient.

[2] Ardern, E. and Lockett, W. T. The Oxidation of Sewage Without the Aid of Filters. (II.) J. Soc. Chem. Ind. 33, 1121, (1915).

[3] Hatton, T. Chalkley. Activated-Sludge Experiments at Milwaukee, Wisconsin, Eng. News 74, 134, (1915).

Quality of Effluents.—The quality of the effluents is usually dependent more on the strength of the raw sewage than upon any other variable. The tanks were filled at 9 A. M., 3 P. M., 9 P. M., and 3 A. M. At 3 A. M. the sewage was weakest and practically all the effluents from the sewage were stable. Many of the effluents from 9 A. M. and 3 P. M. sewages were unstable. Unless the sludge was in good condition stable effluents from the strong sewage were not obtained in 4½ hours by the use of 2 cubic feet of air per gallon of sewage.

Composition of the Effluent Air.—The remarkable purification effected by this process is undoubtedly caused by bacteria, but it is evident that air plays a very important role in the process. An increase in the carbon dioxide and a decrease in the oxygen content of the air during its passage through the tank is to be expected. Comparative analyses of the composition of the air before and after it had passed through the tank were made during a period of three months, from January to April, 1916.[4] The carbon dioxide increased from 4.3 parts per 10,000 to from 19 to 65 parts per 10,000 according to the amount of sludge present in the tank and according to the time after filling the tank with fresh sewage. Ordinarily during a 6-hour cycle, there was a decrease in the amount of carbon dioxide followed by an increase. The decrease was found by experiment with water to be due to the elimination of the dissolved carbon dioxide present in the sewage. At the end of 3 hours the free carbon dioxide was entirely eliminated from the sewage but after this there was a steady increase, owing, undoubtedly, to strong bacterial action. While the carbon dioxide was higher, the oxygen was 1.0 to 1.2 per cent lower in the effluent air than in the influent air. The effluent air contained 19.3 to 19.5 per cent of oxygen showing that about 5 per cent of the oxygen in the air was used up in the process.

Increase of Nitrogen in the Sludge.—Previous investigations[5] have shown that the activated sludge is far richer in nitrogen than sludges obtained by any other sewage purification process and is

[4] Bartow, E. and Crawford, F. N. Composition of the Effluent Air from an Activated-Sludge Tank. J. Ind. Eng. Chem. 8, 646, (1916).

[5] Ardern, E. and Lockett, W. T. The Oxidation of Sewage Without the Aid of Filters. J. Soc. Chem. Ind. 33, 1122-28, (1914).

Bartow, E. and Mohlman, F. W. Purification of Sewage by Aeration in the Presence of Activated Sludge. J. Ind. Eng. Chem. 1, 318-20, (1915).

much richer than is the sludge obtained by direct sedimentation. During 6-hour cycles of operation the sludge present at the end of each 24 hours was collected, dried and analyzed. (See table.)

TABLE

NITROGEN AND PHOSPHORUS IN ACTIVATED SLUDGE FILL-AND DRAW PROCESS. FIVE HOURS' AERATION

Days.	Per Cent Nitrogen (N).			Per Cent Phosphorus (P_2O_5).
	Series I.	Series II.	Series III.	Series III.
0	3.6	3.4	2.9	1.7
1	4.4	3.8	4.3	2.3
2	4.2	4.2		
3	5.3			
4	4.9	4.6	4.4	2.7
5	.....	.....	5.0	2.8
6	.....	4.7	5.1	3.3
7	5.2	4.9	5.6	3.5
8	.....	.....	5.7	3.3
9	4.8			
10	.....	4.6		
11	.....	4.9	4.9	2.8
12	.....	4.9	5.3	2.9
13	.....	4.8	5.1	3.0
14	.....	5.1	5.5	3.1
15	5.2	4.9		
Average excluding first sample	4.9	4.7	5.1	3.0

The amount of nitrogen increased rapidly at first then more slowly reaching a maximum in about one week. Over-aeration caused a drop several times. Omitting the first determination the average amount was 5.0 per cent. The amount of phosphorus (P_2O_5) was determined in one series of sludges. There was a constant increase from 1.7 per cent in the first sludge collected, to 3.1 per cent after 14 days.

1916 *Series of Pot Cultures.*—The wheat cultures of 1915 were grown in pure sand to which the necessary ingredients for plant food had been added. In 1916, in order to imitate natural conditions, a white silt loam and a yellow silt loam from southern Illinois and a brown silt loam from Champaign County were used for three series of wheat cultures. The equivalent of 5 tons per acre of dolo-

FIG. 3.—Fertilizer experiments with white silt loam.

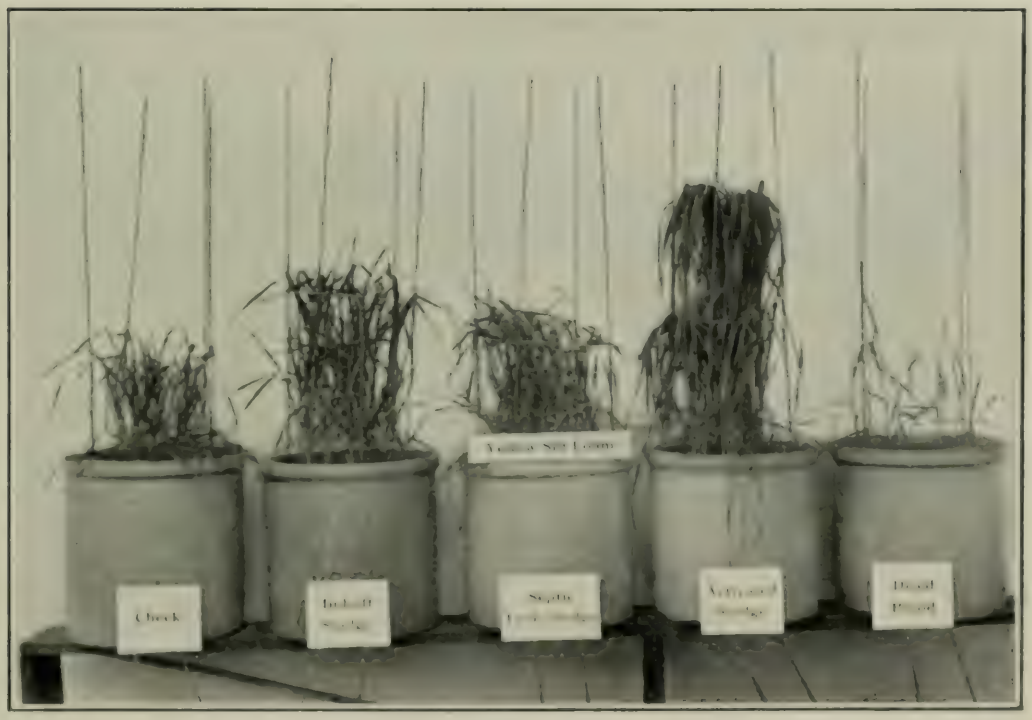

FIG. 4.—Fertilizer experiments with yellow silt loam.

mitic limestone was added to the southern Illinois soils. Each series included a check pot with no sludge or fertilizer and pots containing Imhoff tank sludge from Baltimore, septic-tank sludge from Urbana, activated sludge, and dried blood. Dried blood containing 14 per cent of nitrogen was added at the rate of 1 ton per acre and sufficient of the other materials was used to give an amount of nitrogen equivalent to 280 pounds per acre. At the end of 9½ weeks the wheat in the pots containing the white silt loam and the yellow silt loam fertilized with activated sludge had grown far ahead of that in the other pots. (See Figs. 3 and 4.) The pots containing brown silt loam did not show so great difference, possibly because there was sufficient nitrogen already present in these soils, also because only six seeds germinated. The amount of grain and straw obtained per acre (calculated) shows the best results from activated sludge. With brown silt loam, Imhoff tank sludge and septic tank sludge were detrimental to the yield of grain. With yellow silt loam these sludges were helpful but with the white silt loam the Imhoff sludge was detrimental. The dried blood was equal to the activated sludge when used with brown silt loam but did not give good results with the other soils. This is possibly because of lack of phosphorous and organic matter. These experiments are to be continued.

Continuous Process Plant at the State Water Survey Experiment Station.—The State Water Survey is now operating a continuous flow activated-sludge plant designed to handle 200,000 gallons of sewage and sludge per day. The plant has been built in the septic tank designed by Prof. A. N. Talbot in 1897 for the city of Champaign, Illinois.

THE RECOVERY OF BENZOL FROM GAS

By F. W. SPERR, Jr.

(Chief Chemist, H. Koppers Co., Pittsburg, Pa.)

Read at the New York Meeting, Jan. 12, 1917

One of the most noteworthy achievements in American chemical industry during these last two eventful years has been the rapid development of the recovery of benzol from coal gas. Associated principally with the manufacture of by-product coke, the business of recovering benzol in this country had previously lagged behind the parent industry which was already beginning to make striking progress three or four years before the outbreak of the great war. The lag was due, partly, to a seeming lack of real demand for the material; partly to the abundance of petroleum and partly, also, to the groundless fear that the operations necessary for the recovery of benzol would add too many complications to the manufacture of by-product coke.

Thus, at the close of 1913 there were about sixteen plants in the United States making Light Oil from coke oven gas. Most of the product from these plants was used for the enrichment of illuminating gas leaving a comparatively small amount of material for other purposes. There were only two or three small plants for the manufacture of pure benzene, toluene, etc.

Even at this time, however, there were signs that the importance of the recovery of benzol was gaining recognition, and it is likely that the advancing price of gasolene would soon have led to a large extension of the industry even without the stimulus of war prices.

From the close of 1914 to the summer of 1915 the prices of benzol products rose with the cessation of foreign supply and the ever increasing necessities of the munition business, until pure benzene commanded over 80 cents per gallon and toluene was sold as high as $7 per gallon. The response was rapid. The first complete modern plant for the large scale production of pure benzene and toluene was put into operation in May, 1915, and the total production of benzol

products in the United States in that year rose to over twenty-two million gallons,—more than twice as much as had been produced the previous year.

At present writing (January 1, 1917) there are about forty benzol recovery plants in operation in connection with by-product coke plants, having a capacity of approximately thirty million tons coal per annum. Making due allowance for the facts that a number of these plants scrub only part of their gas and that part of the benzol is immediately used for enrichment, a conservative estimate would place the present annual light oil production at not less than forty million gallons. Three of these plants are now being enlarged, and eight new plants are in course of construction. The completion of this new work will bring the annual production of light oil up to nearly fifty million gallons.

The price of benzene and toluene, although lower than the abnormal figures of two years ago, are still firm, the former at about 55 cents per gallon and the latter at about $1.75 to $2.50 per gallon.

It is to be especially noted that the increase of output has been almost altogether in the production of pure products; the amount of unrefined benzol now being made is probably actually less than was made in 1914. It appears probable that at least 80 per cent. of the present production is redistilled for the preparation of pure benzene and toluene.

The question is frequently asked: "What will become of this vast production of benzol after the war?" It must be remembered, in the first place, that the use of the benzol group of explosives, particularly trinitrotoluol and tetranitroaniline, is by no means confined to military purposes, and their consumption for commercial blasting operations will doubtless increase to a very large extent, because the war has given such a convincing demonstration of their superior qualities. The demand for military explosives is likely to continue on a considerably greater scale than before the war on account of the increased amount of military training that is being required in every country, and on account of the general recognition of the wisdom of accumulating and storing large reserve supplies of the principal explosives. It is one of the peculiar merits of trinitrotoluol that it can be stored indefinitely without risk of deterioration, and the importance of conserving this valuable asset in national defense cannot be overestimated.

The vast development of American chemical industries insures

a market for large quantities of benzol products to be used for synthetic purposes. Benzene, itself, is an indispensable source of nitrobenzene and aniline with its long series of valuable derivatives. The manufacture of synthetic phenol, which is far outstripping that of carbolic acid made directly from coal tar, may be considered as a very important outlet for pure benzene on account of the great increase in the production of the various condensation products of phenol and formaldehyde. From toluene we obtain such well-known products as benzaldehyde, saccharin, and benzoic acid. Metaxylene is the source of a rather important series of dyes. The study of the other materials obtained in smaller amounts in coke oven benzol and available for the production of many interesting series of synthetic chemicals has barely begun but promises to afford striking results in the not far distant future.

Passing thus briefly over the consumption of benzol in the chemical industries, let us consider its employment as motor fuel. So far as bulk is concerned this bids fair to surpass any other possible source of demand. Previous to the war, Germany used over 50 per cent of its benzol production for the operation of internal combustion engines. J. S. Critchley, President of the British Institution of Automobile Engineers, is quoted as stating that the carburation of benzol presents no difficulties and that in actual practice the material gives an increase of mileage of about 20 per cent over gasoline, affording at the same time 12 to 15 per cent more power. A series of tests[1] made in 1913 at Brooklands, England, showed an average of 5 miles increase per gallon.

The demands of the automobile industry alone are so great as to preclude the possibility of any destructive competition between benzol and gasoline. As was stated early in this paper, the benzol recovery plants now in course of construction, together with those already operating, will make an annual production of about fifty million gallons of benzol. There are now over two million automobiles in operation in this country so that the contemplated benzol production would give each machine about 25 gallons. If all the coke that the country now requires were coked in by-product ovens with benzol recovery apparatus we should expect a total annual benzol production of about 110 million gallons or 55 gallons per machine. This total benzol production would be less than half the amount of gasoline and naphtha exported in 1915.

[1] "Gas World," Sept. 6, 1913, Coking and By-Product Section, page 9.

Other uses of benzol may be rapidly summarized for sake of completeness. Quantities are employed in the manufacture of paints, stains, varnishes, and lacquers; in the cleaning industry; in the extraction of grease and fats; as solvents for rubber and for the manufacture of artificial leather and insulating materials. Lastly, may be mentioned one use which should be considered rather as an economic waste than of benefit to the industry. This is for enriching illuminating gas to make it conform with antiquated candle-power standards based on the use of flat flame burners. The utilization of mantle burners and the increasing tendency to replace old candle-power specification by calorific standards, already universally adopted in more advanced European practice, will doubtless soon do away with the necessity for gas enrichment, and make it possible for large illuminating gas plants to recover their benzol and utilize it for more rational purposes.

Before proceeding with the technical description of the benzol recovery process it may be well to give a few moments' consideration to the gas from which the material is extracted. The gases produced by the high temperature distillation of coal are extremely complex in composition, but we may consider their constituents as grouped in two classes, viz., easily condensible and difficultly condensible substances, the former solid, liquid, or very soluble in water at ordinary temperatures, the latter gaseous and difficult to liquefy. Most of the substances of the first group such as water, ammonia, and the heavy hydrocarbons of tar, are removed by the usual operations of condensation and treatment with fresh water or acid. The amount and nature of the easily condensible substances left in the gas after undergoing these operations depends upon the vapor tension of each. Substances of a relatively high vapor tension such as benzene and toluene remain to a large extent, uncondensed. Only 5 per cent. of the total benzol in the gas is removed with the tar in the condensing plant. Actually, of course, the gas after leaving the plant contains slight amounts, in the form of vapor of all the original condensible substances, but the quantities of substances having higher boiling points than that of naphthalene (218° C.) are practically negligible.

Exclusive of water, the easily condensible vapors remaining in ordinary coke oven gas after the usual process of condensation and ammonia recovery, amount to about 1 per cent by volume of the gas. They might be removed by cooling the gas to a very low tem-

perature and this was actually done on a large scale in Europe some years ago, but the method was handicapped by mechanical difficulties largely due to the separation of ice and has been altogether abandoned. The only practical alternative which can be employed to recover these vapors, without altering their chemical constitution, is to treat the gas with a medium in which the vapors are soluble and from which they may be recovered by simple distillation.

This is the principle of every benzol recovery process in operation at the present time. It appears to have been first employed in 1859 by Vogel who washed coal gas with fatty oils for recovering the benzol. It is peculiar that Vogel had only the idea of enriching these oils so as to improve their quality for illuminating purposes. Various processes for extracting benzol based on this principle were later patented; but it was not until 1887 that it was put into successful commercial operation. This was done in Germany by F. Brunk, who is generally acknowledged as being the founder of the modern benzol recovery industry.

It is a peculiar circumstance that the condensible hydrocarbon vapors belong almost entirely to the aromatic series. Benzene together with its homologues, toluene and the xylenes, constitutes over 85 per cent. Other substances present in much smaller amounts are carbon disulphide, thiophene, pyridine, naphthalene, and various olefines of both cyclic and open-chain structure. Members of the paraffin series of hydrocarbons are almost entirely absent. The mode of formation of these compounds in the destructive distillation of coal, the effect of various conditions of carbonization, such as temperature and rate of heating on the character and amounts of the various substances formed, and the variation in the quality and quantity of the products from different groups of coals, are subjects of extreme interest on which our conceptions are not yet by any means clearly defined. The most important generalization that we are able to make is that the maximum yields of benzene and toluene are obtained at moderate coking temperatures. Higher temperatures have a tendency to form more benzene apparently at the expense of the toluene and extremely high temperatures decrease the yields of both benzene and toluene.

The percentage of the more important constituents of typical coke oven Light Oil and the amounts usually produced per ton of coal (2000 lbs.) are given herewith.

TABLE I

AVERAGE COMPOSITION OF COKE OVEN LIGHT OIL

	Per Cent in Light Oil.	Gallons per Ton Coal.
Benzene	66	1.782
Toluene	15	0.405
Xylene	8	0.216
Other substances (principally hydrocarbons)	11	0.297
Total	100	2.700

For sake of convenience a table of the more important properties of some of the constituents of light oil is also inserted. In the preparation of this table the literature has been exhaustively investigated and every endeavor has been taken to present the data as accurately as possible.

In discussing the operation of a benzol recovery plant of modern type I shall deal principally with the Koppers patented process, because I have made a rather special study of this process as being typical of modern practice. To the best of my knowledge all of the coke oven benzol plants now under construction in America, with possibly one or two exceptions, are of this type.

In America the wash oil generally used to absorb the benzol is a petroleum product usually known as straw oil, of which at least 90 per cent should distil between 250 and 350° C. A good absorbent oil has a specific gravity of less than 0.88 at 15° C. and is readily fluid at 4° C. It contains no naphthalene or pitch and exerts a good solvent action on benzol. In best practice the amount of benzol absorbed (technically the "enrichment") is kept between 2 and 3 per cent of the absorbing oil. Too high enrichment is likely to lead to loss of benzol and too low enrichment may involve needless consumption of absorbing oil and steam. In European practice heavy tar oils are used almost exclusively as absorbing media and such materials may find increasing application in America. Such tar oils should contain less than 7 per cent naphthalene and 90 per cent of the material should distil between 200 and 300° C.

Figure 1 shows a flow sheet of a benzol plant showing the principal features of operation. Let us follow first the course of the gas and then that of the oil. Previous to treatment with the absorbing oil, the gas should be cooled to a suitable temperature

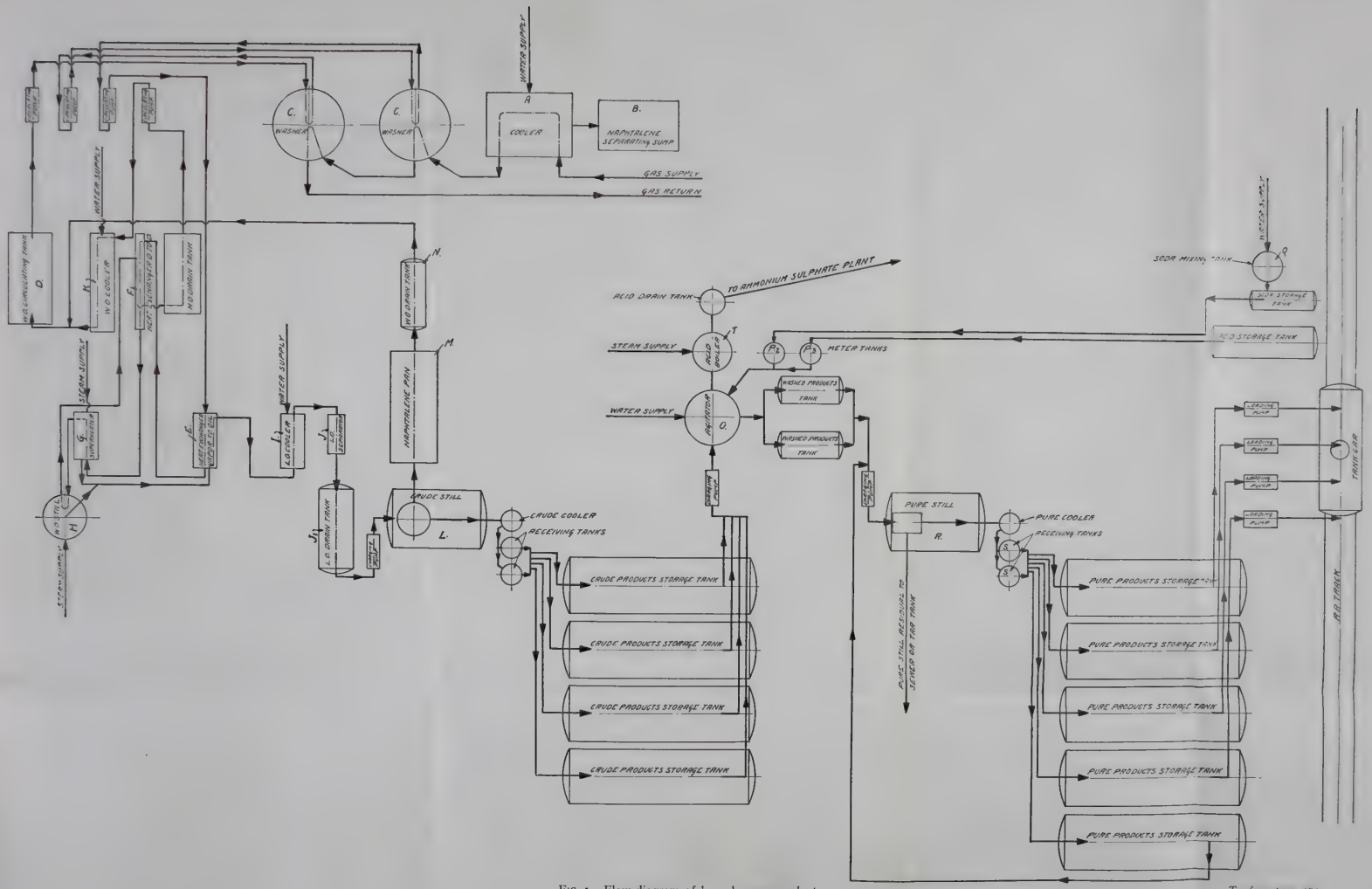

FIG. 1.—Flow diagram of benzol recovery plant.

To face page 174.

which may vary from plant to plant and depends principally upon such factors as its moisture and naphthalene content, temperature of the wash oil, and per cent. enrichment desired. This cooling is accomplished by means of the cooler (*A*), which is preferably of the direct contact type. The water not only acts as a cooling medium, but mechanically washes a large portion of the naphthalene out of the gas and carries it into the separating sump (*B*). The cool gas then passes into the benzol washers (*C*.) These are tall scrubbing towers of the hurdle type, effecting a very intimate and prolonged contact between gas and oil. The debenzolized gas passes out of the last washer through the return main to its point of consumption.

The fresh wash oil is pumped from the circulating tank (*D*) over the scrubbers in an opposite direction to the flow of the gas, maintaining the counter current principle that should be adopted in nearly all scrubbing operations and bringing the fresh washing medium into the scrubbing system at a point where the gas contains the least light oil vapors. The distribution of the wash oil over the tops of the scrubbers is a very important matter, and should be done as uniformly as possible. The enriched wash oil accumulates in tanks usually located underneath the scrubbers, and is pumped from these to the benzol recovery plant to be heated for the purpose of releasing its benzol constituents. Part of this heating is accomplished in the Koppers System by the utilization of the heat in the hot debenzolized wash oil leaving the still. Two heat economizers are used in this system. The cold oil first enters the heat exchanger (*E*), where it is heated by benzol vapors and steam from the still (*H*), thence it is conducted to a second heat exchanger (*F*), where it receives additional heating by means of hot debenzolized wash oil leaving the still (*H*). It is then still further heated to the maximum temperature desired by means of live steam in a superheater (*G*), from which it passes into still (*H*). This still is composed of a series of superimposed sections or chambers as in common distillation practice. The heated oil flows down through these sections while steam is blown directly into the lowest section and travels in a direction opposite to that of the oil. The mixture of benzol and water vapor is partially rectified in the upper portion of the still, and then enters the heat exchanger (*E*), as mentioned before, where it is condensed and the partial preheating of the enriched wash oil is effected.

TABLE II

COMPILED BY THE LABORATORY OF THE H. KOPPERS COMPANY, MELLON INSTITUTE, PITTSBURGH, PA.

	Carbon Disulphide		Benzene		Toluene		M-Xylene		Naphthalene	
	Data.	Ref.	Data.	Ref.	Data.	Ref.	Data.	Ref.	Data.	Ref.
Formula (Molecular).	CS_2.		C_6H_6.		C_7H_8.		C_8H_{10}.		$C_{10}H_8$.	
Molecular weight (o = 16)	76.12	1	78.05	1	92.06	1	106.08	1	128.06	1
Lbs. per U. S. Cal. (60° F.)	10.57	2	7.36	3	7.27	8	7.26	8	9.60	5
Sp.gr. (0° C./4° C.)	1.2921	2	0.8999	3	0.8845	4	0.8823	8	.....	11
Sp.gr. (10° C./4° C.)	1.2773	2	0.8893	3	0.8757	8	0.8738	8	.....	11
Sp.gr. (15° C./4° C.)	1.2698	2	0.8839	3	0.8714	8	0.8697	8	1.1517	5
Sp.gr. (20° C./4° C.)	1.2623	2	0.8786	3	0.8659	3	0.8655	6	.....	11
Sp.gr. (30° C./4° C.)	1.2473	2	0.8679	3	0.8573	8	0.8574	8	.....	11
Expansion coefficient (° C.)	0.00125	2	0.0012	7	0.0010	7	0.00095	7	.....	11
Boiling-point (760 mm. Hg)	46.2	1	80.36	12	110.3	13	139.1	13	217.7	1
Increase in boiling-point (°/mm. Hg)	0.041	14	0.043	15	0.047	15	0.052	15	0.059	16
Vapor pressure mm. Hg (0° C.)	127.9	14	26.63	18	7.20	18	1.75	18	0.022	9
Vapor pressure mm. Hg (10° C.)	198.5	14	45.68	18	13.02	18	3.45	18	0.047	9
Vapor pressure mm. Hg (15° C.)	244.1	14	58.90	18	17.22	18	4.74	18	0.062	9
Vapor pressure mm. Hg (20° C.)	298.0	14	75.21	18	22.53	18	6.43	18	0.080	9
Vapor pressure mm. Hg (30° C.)	434.6	14	119.34	18	37.46	18	11.43	18	0.135	9
Lbs. per cu. ft. vapor (60° F.–30″)	0.202	10	0.209	10	0.244	19	0.281	19	0.339	20
Kg. per cbm. vapor (0° C.–760 mm.)	3.42	10	3.54	10	4.14	19	4.76	19	5.72	20
Heat combustion (net) 15° C.–760 mm. Hg.										
Cal. per kg. liquid	3,480	10	9,960	17	10,150	17	10,230	17	9,700	17
Cal. per liter liquid	4,420	21	8,805	21	8,850	21	8,910	21	11,170	21
B.T.U. per lb. liquid	6,260	21	17,930	21	18,270	21	18,410	21	17,460	21
B.T.U. per U. S. gal. liquid	66,100	21	132,100	21	132,600	21	133,500	21	167,300	21
Cal. per cu.m. vapor	11,550	21	33,600	21	40,150	21	46,500	21	52,400	21
B.T.U. per cu.ft. vapor	1,300	21	3,780	21	4,500	21	5,210	21	5,910,	21
Specific heat (Cal. per kg.)	0.240	23	0.419	24	0.440	24	0.383	25	0.314	26
Heat vaporization (Cal. per kg.)	83.8	27	92.9	27	83.55	28	78.25	28	.....	11
Solubility in water (22° C.):										
Gms. substance in 100 gm. H_2O	0.219	22	0.072	22	Insol.	1	Insol.	1	Insol.	1
Gms. H_2O in 100 gm. substance	0.765	22	0.241	22	Insol.	1	Insol.	1	Insol.	1
Melting-point (° C.)	−108.6	1	+5.4	1	−92.4	1	−54.8	1	+80.0	1

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NOTE.—References are, in many cases, to the fundamental data from which constants were calculated, i.e., (2) refers to relative volume data; (10) was computed from vapor density, molecular heat, etc.

The remaining vapors are completely condensed and the total condensate cooled in a water-cooled condenser (*I*). The light oil is separated from the water in the condensate by means of the separator (*J*).

The debenzolized wash oil after leaving the still passes through the heat exchanger (*F*), where it gives up a part of its heat to the enriched wash oil, as stated above. Then it is finally cooled in the water cooler (*K*). The cool oil is then delivered to circulating tank (*D*), thus completing the cycle.

The improvements that have been made in recent practice have to do principally with effecting as great economy as possible of heat in the cycle of gas treatment, distillation, and cooling, through which the wash oil passes. In the Koppers System it is calculated that the devices for heat economy reduce the steam consumption in distilling the enriched wash oil by more than 80 per cent. 1000 gallons enriched oil 77 to 284° F. (1000×7.3×.537×207)= 811,440 B.T.U.=840 lbs. of steam at full pressure, or per 1000

gallons purified benzols, 55×840=46,200 lbs. of steam which, at 2 cents per 100 lbs., costs $9.24. Of this steam, heat exchanger

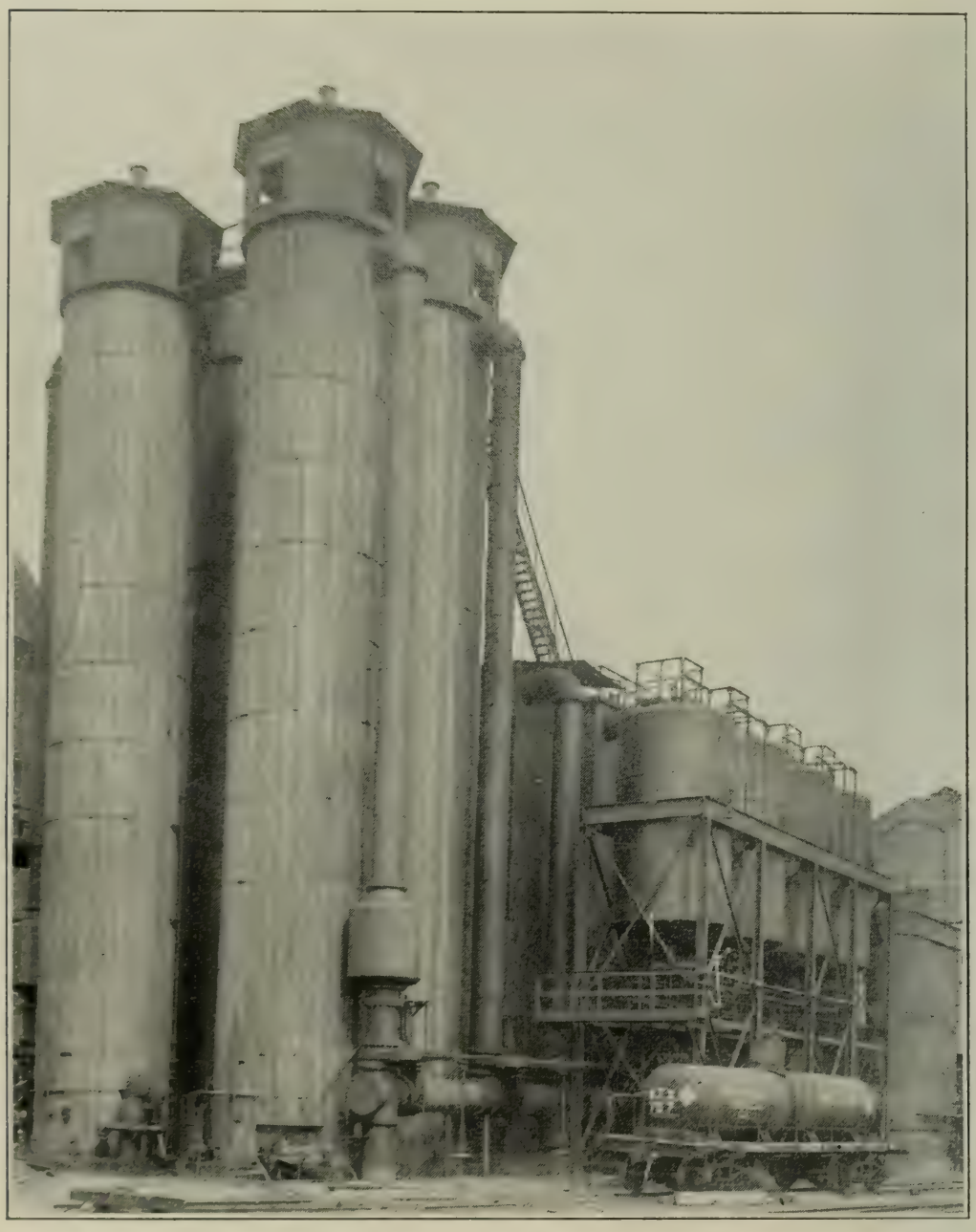


FIG. 2.—Gas coolers and benzol scrubbers.
H. Koppers Co., Pittsburgh, Pa.

(*F*) cooling the hot debenzolized oil from 275 to 170° F. in preheating the enriched oil, saves per 1000 gallons enriched oil (973

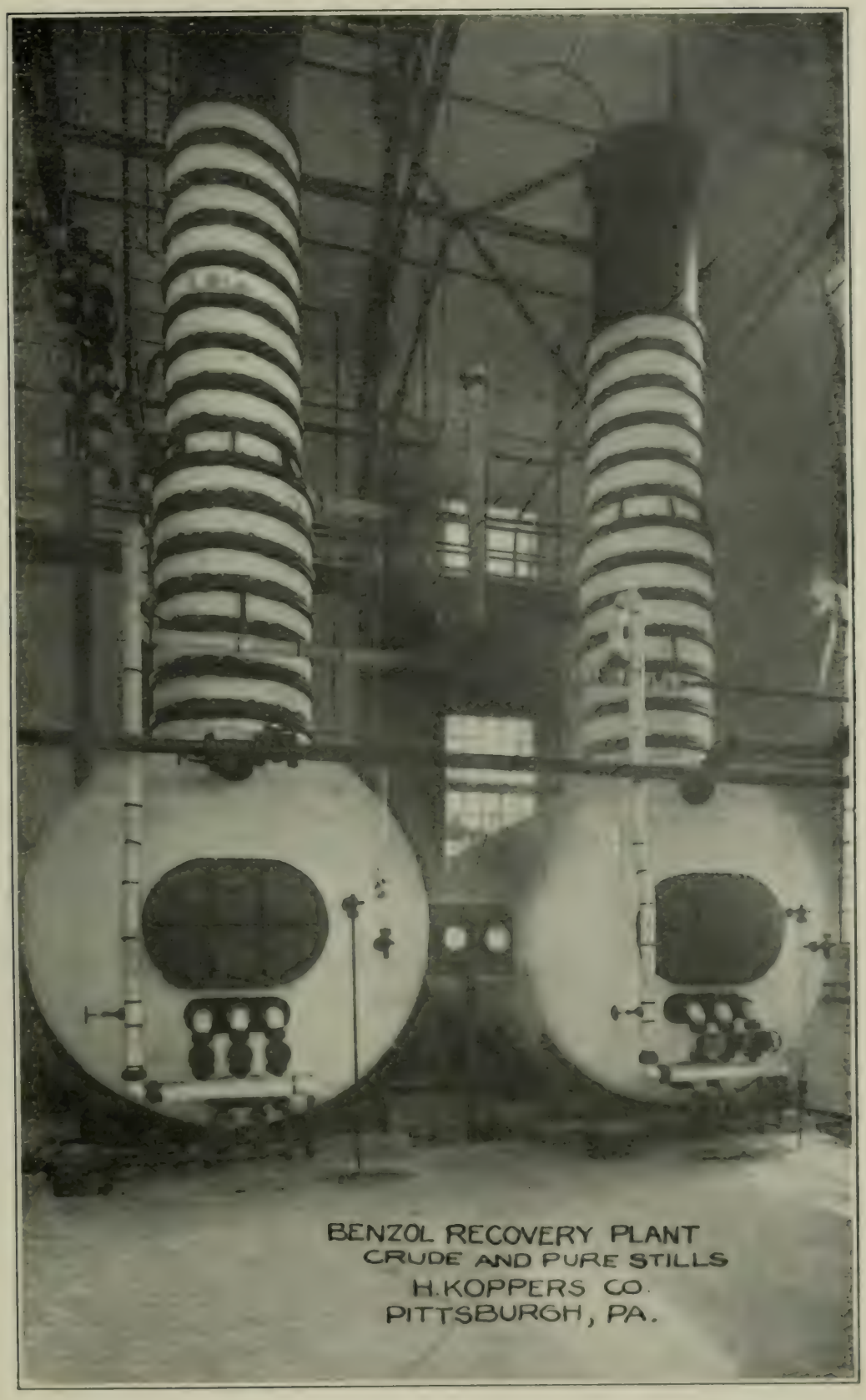

BENZOL RECOVERY PLANT
CRUDE AND PURE STILLS
H. KOPPERS CO.
PITTSBURGH, PA.

FIG. 3.

gallons debenzolized oil), $973 \times 7.3 \times .54 \times 105 = 402{,}675$ B.T.U.= 417 lbs. of steam or 49.8 per cent of the total; heat exchanger (E) heating the cold, enriched oil by the benzol vapors and steam from the still, adds a recovery of about 256,000 B.T.U. per 1000 gallons wash oil, equivalent to 265 pounds of steam or 31.5 per cent of the total; in the two heat exchangers, therefore, the total saving is $417+265=682$ lbs. of steam per 1000 gallons of enriched wash oil circulated, or 37,510 lbs. of steam for every 1000 gallons of purified benzol produced, and the cost of steam for this purpose is reduced from \$9.24 to \$1.72 per 1000 gallons of product.

Further economy lies in the saving of cooling water circulation which would be required to cool the debenzolized wash oil.

The light oil is accumulated in a drain tank (J) shown in the lower part of the figure, and portions are taken for distillation in still (L). This still is usually known as the crude still, the first distillation of the wash oil being made for the purpose of effecting an approximate separation of several fractions of different boiling points preliminary to washing and final rectification. This and subsequent distillations are made intermittently in stills of large capacity (6000 to 12,000 gal.), which give better fractionations than are possible in smaller apparatus. The continuous type of stills has not been found satisfactory in this work. The crude still does not require the elaborate dephlegmator, that is necessary on the final rectifying stills. The heating is accomplished by means of internal steam coils and a direct steam spray. The benzol and toluol are principally distilled off by indirect heat, using the steam coils, and the higher boiling constituents xylol, solvent naphtha, etc., are then distilled over by introducing steam directly into the still.

After the benzol, toluol, xylol and solvent naphtha have distilled off, a certain amount of wash oil containing naphthalene remains in the still tank. The presence of wash oil in the light oil is due not only to mechanical trapping of the heavy oil during the distillation, but also to the actual distillation of some of its original constituents by agency of the direct steam used. The products recovered from 1000 gallons of light oil vary, according to the kind of coal coked, the regulation of the ovens, and the method of operation of the light oil plant. In one plant that may be taken as fairly typical of a well-operated system the yields average about as follows.

From 1000 gallons crude light oil:

680 gallons crude benzol.
140 gallons crude toluol.
50 gallons crude xylol.
55 gallons solvent-naphtha.
75 gallons wash oil residue with naphthalene.

The wash oil remaining in the still is drained into cooling pan (*M*), where it is cooled in the air to crystallize out the napthalene. The wash oil is drained away from the latter into tank (*N*), and then is returned to the main circulating tank (*D*). In large plants where the amount of naphthalene is great, a centrifugal dryer is employed for the purpose of separating the small amount of oil remaining in the naphthalene, and also for reclaiming the naphthalene from the separating sump at the foot of the gas cooler. The crude naphthalene so obtained can be sold at such, or may be put into the tar in the coke plant.

The products obtained from the crude still will satisfy many commercial purposes in normal times. However, at present the demands of chemical manufacturers for benzol and toluol of a high degree of purity have made it advisable to accomplish the complete process of purification at the coke plant to serve this important part of the trade. For this purification the crude benzols are first washed with sulphuric acid and then with caustic soda and water. This operation is accomplished in agitator (*O*), which is a large lead-lined vessel with an efficient mechanical mixing device for bringing the acid and benzol into intimate contact. The acid is commercial concentrated sulphuric acid (66° Bé.). The quantity used is accurately measured from the meter tank (p_1). The caustic soda solution is prepared in the tank (*Q*) and measured in the meter tank (p_2).

The acid has the effect of reacting with and to a large extent polymerizing most of the impurities which consist of various olefines and substances of similar character, together with certain phenoloid bodies. This results in the formation of resinous substances of very high boiling point, part of which are insoluble in the benzols and settle out with the acid in the bottom of the agitator, while part go into solution, giving the benzol a dark brown or reddish color. The acid sludge is drawn off and treated as will be described later. The caustic soda neutralizes any traces

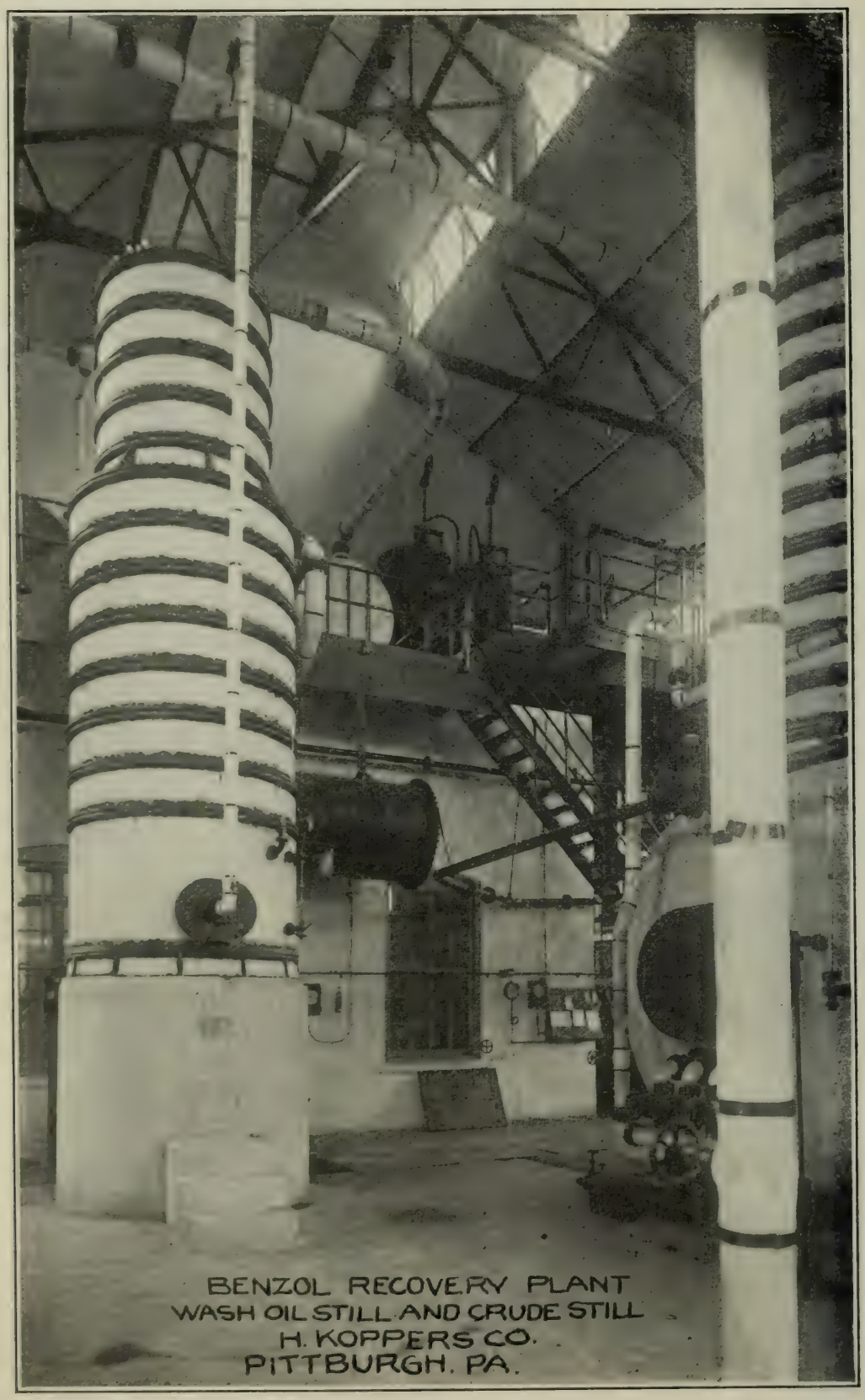

BENZOL RECOVERY PLANT
WASH OIL STILL AND CRUDE STILL
H. KOPPERS CO.
PITTBURGH. PA.

FIG. 4.

of acid which may remain in the agitator and effect the removal of some of the phenoloid bodies. After the soda wash, the benzol has a lighter brown color, but always requires distillation. The washed benzol is delivered from the agitator to the still (*R*). This still is generally of the same capacity as the crude still, but is provided with a very efficient dephlegmator. Sometimes purified products of less exact boiling points are desired. In this case the washed benzols are distilled rapidly, simply to separate the benzol from the resinous materials in solution. The products so produced are termed "purified," e.g., 90 per cent benzol, 50 per cent pure benzol, etc., the nomenclature being based on the percentage in test distilling under 100° C. The distillation for the preparation of pure products is conducted more slowly, the condensate being collected in receivers (*S*), and tested carefully before placing in the final storage tanks.

Great improvement has been made in recent practice in the distillation of pure benzol and toluol. In a well-designed plant the cuts are remarkably clean and the percentage of intermediate fractions small. At one benzol plant, pure benzene is regularly being produced of a grade such that less than 5 per cent distils below 80.1° C. and 95 per cent distils within 0.3° C. Pure toluene also is being made of which less than 5 per cent distils below 110.0° C. and 95 per cent within 0.5° C. This extraordinary degree of purity is of great advantage to manufacturers of explosives and synthetic chemicals requiring the use of pure benzols. The ease with which these distillations can be effected, and the sharpness of the cuts obtained is very remarkable. Much of the secret of success lies in the correct design of the dephlegmator. The record of one plant shows the following average figures for the distillation of crude benzol containing toluol:

Pure Benzene	85.3%
Intermediates	3.4%
Pure Toluene	9.8%
Residue	1.3%
Loss	0.2%

The residue is often allowed to go to waste; but may be mixed with the coke over tar without injury.

The water and caustic soda used in the agitator are drained to the sewer. The acid sludge drained from the agitator is de-

livered to a boiler (*T*), in which it is treated with direct steam. This effects a separation of the resinous materials in the form of a heavy carbonaceous, spongy deposit. An acid of about 40° Bé. is recovered and may be used on the coke plant for making ammonium sulphate. The boiler is covered during the operation of steaming and the escaping vapors are condensed in a cooler.

Regarding other details such as the arrangement of pumps, storage tanks and piping, the diagram is self-explanatory.

The upkeep of a benzol plant costs remarkably little, and the labor required in operation is small. A complete plant handling 5000 gallons of light oil per 24 hours is operated by 5 men on day turn and 3 men on night turn, with 2 chemists for control testing and 3 laborers on day turn only for loading shipments and for general utility purposes.

The apparatus that is subjected to the most severe conditions is the superheater. Spare superheaters should always be provided and the apparatus arranged so as to be readily interchanged. All apparatus for heating and cooling the wash oil should be so arranged that each individual unit can be taken out easily without disturbing the other apparatus. All apparatus with the exception of that exposed to sulphuric acid is made of iron and steel, no special alloys being required. It is very essential that the utmost precaution be taken in the arrangement of the piping to obviate the possibility of accidental mixing of the different products.

Since benzol is a by-product of coke and gas making, those items in the cost of its production which are involved also in coke and gas production are not chargeable to the benzol. In other words, the cost of making coke oven benzol includes only the cost of its extraction from the coke oven gas, and that of its purification. This cost in the United States will vary according to local conditions, but usually lies between 4.0 and 7.0 cents per gallon.

At the close of the European war, when prices again become normal, it will be possible with the large plant capacity then available, to make a substantial profit from the production and sale of automobile benzol (90 per cent. purified) at prices competing with those of gasolene. There are other large uses creating a demand for the various benzols, described in earlier paragraphs, and further uses will be developed as the supply of the material becomes greater.

The effect of the removal of benzol on the calorific value of

the gas has been well worked out by J. W. Shaeffer, who recently published his results in an article read before the October, 1916, meeting of the American Gas Institute. The actual loss in the calorific value of the gas amounts to about 5.8 per cent. which is a figure representing the average practice of about thirty by-product coke plants. The figure agrees well with theoretical considerations. The result is that more debenzolized gas has to be used for accomplishing a given heating effect. Assuming that one ton of coal makes 11,000 cubic feet of gas, a reduction of 5.8 per cent, is equivalent to 638 cubic feet less gas. As boiler fuel this gas is worth about six cents per 1000 cubic feet, so that the reduction may be figured to cost about 3.83 cents per ton of coal, i. e., about 8 per cent of the normal value (based on gasolene prices) of the total benzol recovered from a ton of coal.

DISCUSSION.

The CHAIRMAN: Gentlemen, you have heard this very interesting paper, which is now open for discussion.

F. W. FRERICHS: Mr. Chairman, I would like to ask one question, which perhaps I should have asked this morning in connection with Mr. Campbell's paper. It refers to the refining and separation of benzol and toluol. I am informed from a most excellent source in Europe that the Raschig column is more and more coming into favor there, and that this column is employed with great success for the separation of benzol and toluol. This Raschig column consists of an upright cylinder closed on top and bottom and this column is filled with short cuts of iron pipe of different diameters. They use one, two and three-inch pipe cuttings, the length of each pipe being equal to the diameter of the pipe. With such cuttings they fill up the entire column and run the liquids to be separated by distillation in a small stream from top to bottom of the column. The heat is applied from below in the usual way, by indirect steam, and separation of the liquids is effected in this apparatus with just as good an efficiency as in the most complicated column. This column goes under the name of the Raschig column, the inventor being a well-known manufacturer of carbolic acid.

F. W. SPERR: I am not especially familiar with that type of column, but there seems to be a tendency in present practice, to make distilling columns simpler, and to depend more on the opera-

tion of the dephlegmator. I do not know what type of dephlegmator they have with this column. The performance of such a column is an interesting confirmation of what we find in the laboratory. The work of Rittman[1] showed that in the long run the old-fashioned Hempel column was perhaps better than any other. The result of laboratory practice is that we are throwing away the more complicated columns, in favor of the simple Hempel column, and are getting extraordinarily sharp fractionations with it. That Raschig column is simply a large scale Hempel column.

Dr. ITTNER: With reference to this column, the description of it in the patent is that these ferules, as I might call them, are made, or intended to be made, exactly the same in diameter as they are in length, the idea being that when they are thrown into the column they will not take any particular position. They assume every possible position and will not retain much liquid and will present a very large amount of surface. This, as the speaker said, is very similar to the old Hempel column. I don't know where the idea originated, although it has been patented since I have seen it in use. It was recommended by Prof. Henry B. Hill, of Harvard College, to my certain knowledge, more than twenty years ago, because I used it there myself. Instead of using glass beads he took glass tubing and recommended that it be cut up in this way. This column is exactly the same thing we used to make with glass tubing years ago, and is more efficient than if made with glass beads. That column is patented in this country.

CHARLES L. CAMPBELL: I am familiar with that patent and with that system of distillation. I am not familiar with it from actual results or actual test, but it seems to me very possible that such a column might be very efficient indeed. I can well understand it. The principle of the column, and the efficiency, depends on the intimate mixture of the vapors and the liquid, and in the successive number of steps, the greater the number of steps the more efficient the column; and it is very possible that with a relatively short column of that type it would be possible to get very efficient mixtures, and the same result that we might get with a high column with a very large number of plates. As I say, that seems to be possible, and from what I have heard I am inclined to think that such results have been obtained.

F. M. FREICHS: These short sections of 1, 2 and 3-inch pipe

[1] W. F. Rittman and E. W. Dean, Jour. Ind. & Eng. Chem. 1915, p. 754.

may be replaced by simple sheets of iron, which are bent together in a way similar to these short sections of pipe, and being open on one side.

F. W. SPERR: In one small type of still column which we found to be very efficient we used little steel ball bearings. In applying that principle to a large still there is one thing that you have to be careful to avoid, and that is the danger of by-passing or irregularity of distribution in some part of the apparatus. It is always well to have arrangements throughout the column to relieve any tendency to by-pass.

Mr. PETERSON: I would like to ask if it is possible to recover benzol from producer gas, particularly clean or washed producer gas, such as is made in the Smith gas producer.

F. W. SPERR: That question came up about two years ago, and I investigated it on one type of producer which I think is very similar to the Smith, and was not able to find any benzol worth recovering. I don't believe, unless you would use a very rich bituminous coal, that you would get any benzol from producer gas—it would not be worth the trouble.

Mr. PETERSON: It seems to me that it might be possible to recover the ammonia from such gas.

F. W. SPERR: That is different. The possibilities in this direction are great and the recovery of ammonia from producer gas is just in its infancy. The amount of ammonia recoverable from producer gas puts our coke oven practice in the shade, so to speak. A ton of coal in a coke oven will yield, say, 25 pounds ammonium sulphate while if you gasify the same coal in the right type of producer you can get 80 to 85 pounds.

F. W. FRERICHS: One type of that kind of producer has been used in Springfield. I have used several hundred thousand pounds of their ammonium sulphate which was obtained from common Illinois screenings. This coal produces as much as 60 or 65 pounds of ammonium sulphate per ton of coal.

The CHAIRMAN: Is there any further discussion?

Dr. ITTNER: A few years ago when we were out at Detroit we went through a plant, the power of which was furnished by producer gas, and the company making the producer gas got ammonia enough from the gas to pay for the coal.

DEVELOPMENT IN CHEMICAL ENGINEERING EQUIPMENT

By H. D. MILES

(Pres. Buffalo Foundry & Machine Co.)

Read at the New York Meeting, Jan. 10, 1917

As it is impossible in the time at my disposal to adequately describe the development of chemical engineering equipment, I shall only briefly review some of its more important phases as they have come under my observation in the manufacture of chemical equipment, by the Buffalo Foundry & Machine Company, of which company I am the president.

This company was organized sixteen years ago on what at that time was considered a radical departure, namely: the operation of a modern foundry on a scientific laboratory-controlled basis. At that period, the large majority of foundries were operated by rule of thumb methods, and no attention was paid to the chemical composition of the metals used. Metal mixtures were made on a grade basis, known as Nos. 1, 2, 3 and 4, gray forge, scrap iron, etc., the grade being determined by the appearance of its texture rather than by its chemical composition. Very little attention was paid to the percentage of different elements contained in the iron. Frequently, what appeared to be a high grade No. 1 Pig Iron, would, when analyzed, prove to be an inferior grade.

This laboratory control established a reputation for quality and accuracy, and chemical castings, demanding the highest developed standards, immediately became one of our principal products.

It is now fourteen years since we entered this field, and for an active beginning joined issue with European manufacturers who were exporting into the United States the large pots used by caustic manufacturers, and who largely dominated the American market as the domestic product was inferior. After careful research and development, assisted by the co-operation of our customers, we were finally able to produce caustic pots of a better

quality, that is, giving longer life than those imported from Europe.

We next turned our attention to chemical retorts of one type or another for handling different acids, and to acid eggs, and acid kettles, and ultimately obtained equal results in the manufacture of these.

After our success with these lines of chemical equipment, and several years before the outbreak of the present European war, we entered the field as manufacturers of vacuum dryers, pumps, and condensers, which apparatus has to-day so wide an application in the chemical field. We developed a full line of vacuum dryers, consisting of shelf dryers, rotary dryers, drum dryers, and vacuum drying and impregnating equipment, as well as the auxiliaries which go with such apparatus, namely: vacuum pumps, condensers, expansion tanks, reclaiming devices, dust collectors, etc.

This equipment which we have highly developed with new and patented devices, is especially useful where rapid drying free from atmospheric variations is required, or where in order to avoid injury to the materials under treatment, drying must take place at low temperatures.

In this connection I wish to call attention to our Vacuum Drum Dryers which are constructed to dry in large quantities and at low cost without practically any loss of material, emulsions, pulps, extracts, white lead, glue, milk, acids, dyes, and other liquids containing solids. These liquids are reduced to dry forms economically and quickly and without danger of injury to the quality of the material.

We have so far manufactured five sizes of Vacuum Drum Dryers, the smallest having a drum 2 feet in diameter by 20-inch face, the largest 5 feet in diameter by a 12-foot face. All sizes are so constructed that they may be easily cleaned and kept in sanitary condition, which is a vital consideration when handling food products. The smallest dryer is so constructed that the casing over the drum can be readily moved back on a track so that free access can be had to all parts of the interior. The larger types are so arranged in size and convenience that a man can enter the casing and scour all parts of the interior.

Our patented method of applying the liquid to the surface of the drum is the result of many developments in our particular machine, and the present arrangement insures a very uniform

coating on the drum as well as a large output. There is a large reservoir in the bottom of the casing for holding a considerable quantity of material to be dried. This material is taken, as required, by a circulating pump located beneath the dryer and delivered to a supply pan immediately under the drum and slightly in advance of the knife or scraper which removes the material. Only a small section of the drum surface comes into contact with the liquid material and, therefore, over three quarters of the drum surface is active at all times in drying. The material is forced in the pan under sufficient pressure to insure a uniform coating on the drum. The extra supply of liquid, which escapes on the side of the pan, where the film comes out, overflows into the reservoir in the bottom of the dryer. By the time the material reaches the scraper it is dry enough for removal. It is automatically scraped from the drum, falls into a conveyor and from there is removed to a receiver. Two receivers are used, one on each end of the machine. While one is being loaded, the other is unloaded, an arrangement which permits the machine to be operated continuously.

Where the drum dips into the main body of the liquid, it is impossible, owing to the agitation and foaming of the liquid, to always maintain a constant level of the liquid material. This constant change in level results in an irregular immersion of the revolving drum surface, with a consequent variation in the moisture content of the dried product, loss of continuous service, low output and heavy cost of operation.

With out method, the drum does not come into contact with the main body of liquid, and, therefore, any material can be dried satisfactorily whether it foams in a vacuum or not. It is also to be noticed that as some materials give over their moisture more readily than others, the speed of the drum, and the pressure of steam used in the drum, can be varied to suit the material being treated.

One of the most extensive uses for the "Buflovak" Vacuum Drum Dryer has been in the production of powdered chestnut and hemlock extracts. Formerly, these extracts were put on the market in a liquid state, containing approximate 50 per cent moisture. They were shipped in tank cars. These materials, with this moisture content, can be treated in our machine and dried to a powdered form, having from five to seven per cent moisture content. This dried material can be packed into bags and shipped in this manner

to any part of the world. By handling it in this form the market for the material is not only very much widened, but the expense and trouble of tank cars is eliminated, and consequently transportation expenses greatly reduced.

Another important use for this machine has been developed in the recovery of dry sulphite waste from the waste liquors of paper mills. This product, owing to its recovery in a dry state, now brings a handsome profit to the paper mills, and secures another by-product that can be converted to many useful purposes. It further enables the paper mills to comply, actually at a profit, with the laws prohibiting the pollution of streams with sulphite waste.

The approximate capacity of our 5 foot by 12 foot vacuum drum dryer on the above-mentionel extracts and sulphite waste, is 1000 pounds of dry product per hour. The cost of operation is extremely low. We have dried products where the cost, including depreciation and interest on investment, steam, power, labor, and incidentals, has not exceeded one-tenth of a cent per pound in reducing a product from a 50 per cent moisture content to a commercially dry product under a 7 per cent moisture content.

Vacuum Shelf Dryers and Vacuum Rotary Dryers have also been extensively used in the recent chemical development in this country. They can be used to great advantage in drying some of the chemicals derived from coal tar distillates. This use is in addition to their previously extensive application in the drying of both liquid and solid materials. These dryers are also especially valuable in handling solid materials; the Shelf Dryer for drying sheet rubber, fruit, albumen, etc., and together with the Rotary Dryer, is available for use in drying reclaimed rubber, rubber compounds, paints, dyes, extracts, pastes, glue, soap, salts, starch, vegetables, etc. Where the quality of the material is not affected by being tumbled or mixed while drying, or is not of too sticky a nature, the Rotary Dryer is an ideal apparatus for drying in large quantities.

At the outbreak of the present European war, when the supply of many materials never made in America was cut off, there was a sudden and large demand for chemical equipment, particularly in plants handling the many products produced from coal tar. In order to meet this demand, it was important, to first make use of apparatus developed for these purposes in Europe. It was also

essential to design new types of apparatus to suit conditions in this country and to make modifications in equipment which had been developed abroad.

The Buffalo Foundry & Machine Company, to its already well-established Engineering and Consulting Staffs, at once added a corps of experienced specialists, who, with the co-operation of chemical engineers associated with chemical plants, developed machinery to manufacture economically the new products so greatly in demand, and we are to-day supplying apparatus for the manufacture of a large number of these products, such as

Aniline,
Acetanilide,
Anisidine,
Alpha-Naphtylamine,
Alpha-Sulfonic Acids,
Amido-Naphthol,
Alpha-Naphthol,
Benzoic Acid,
Benzaldehyde,
Beta-Naphtylamine,
Beta-Naphthol,
Beta-Sulfonic Acids,
Carbolic Acid,
Dinitrobenzol,
Dinitro-Chlorbenzol,
Dimethylaniline,
Diphenylamine,
Gamma Acid,
H Acid,
Meta-Nitraniline,
Meta-Phenylene-diamine,
Meta-Toluylene-diamine,
Metanilic Acid,
Naphtionic Acid,
Ortho-Anisidine,
Ortho-Phenetidine,
Para-Anisidine,
Para-Phenetidine,
Para-Nitraniline,

Para-Phenylene-diamine,
Para-Amidophenol,
Phenetidine,
Picric Acid,
Sulphur Black,
Sulfonic Acid,
Toluidine,
Xylidine,
Trinitrotoluol (TNT),
Nitric Acid,
Caustic Soda, etc.

One of the first products we were called upon to produce machinery for, was Synthetic Phenol. The method generally used in treating Sodium Benzol Sulphonate before fusion with the Caustic Soda, was to take the liquor from the filter presses, evaporate it as far as possible in open pans containing heating coils and then dry the salt before fusion, either by means of heated air or by some other crude method. Seeing the field for improvement here, our laboratory started work on this problem, and it may be interesting to you to know the evolution which finally led up to the present method of operation.

We, of course, easily determined that a vacuum evaporator should be installed for reducing the liquor to a certain density, thus doing away with the open pans. The problem that presented itself, was how best to handle the evaporated liquor. Our Engineers and Chemists assumed that the Vacuum Drum Dryer would be the ideal machine for treating the liquor as it came from the evaporator, but we found by actual experiment, that owing to the peculiar nature of the liquor, it was very difficult to secure under a vacuum a good coating on the drying drum.

We next tried the Vacuum Rotary Dryer, but found that this machine formed the product into large cakes or balls. It was impossible to extract the moisture from the interior of these masses without spending considerable time in drying, our object here being, as in all cases, to produce the most economical method for handling a given product. We found that the experience we were having with this product coincided with that which we had had on some other products, which we worked out by using an Atmospheric Drum Dryer for reducing the moisture content to—say approxi-

mately 12 per cent.—and then finishing the process in a vacuum rotary dryer. We found that this experiment produced the desired results and furnished a beautiful Sodium Benzol Sulphonate Salt ready for fusing with the Caustic Soda. Later experiment shows that it was not necessary to reduce the moisture content in the Sodium Benzol Sulphonate to below 12 per cent before fusing with the Caustic Soda, with the result that the Vacuum Rotary Dryer has been eliminated, and the Atmospheric Drum Dryer is used exclusively for handling the liquor as it comes from the evaporator.

We later found that the "Buflokast" Atmospheric Drum Dryer has many uses in chemical plants. The patented successful principles of operation in this dryer are the causes for its exceptional success in this field. It embodies the principles of the "Buflovak" Vacuum Drum Dryer. The same patented automatic device for applying the liquid to the drum, produces a uniform coating on the drum, and consequently a uniformly dry product. It is an ideal machine for drying materials that can be dried rapidly and without injury under atmospheric conditions, such as Salts of Sulfonic Acid, Sodium Naphthalene Sulphonate, and other similar products. The machine is continuous in its operation, is a great labor saver, and the finished product produced is uniform, and when desired for fusion, is in excellent condition to go direct from the dryer to the fusion operation.

One of the early demands since the European war has been for a reducer for the manufacture or Aniline Oil. The improvements here which have been made, are the improved sludge unloading device which automatically raises the valve when open, and wedges it in position when closed; the introduction of reversible liner plates, hollow shaft and rake for the introduction of steam when desired, and the jacketed bottom for special cases.

Another development we have made is apparatus for the distilling of Beta Naphthol. This consists of a still body, constructed of a special metal, and fitted with a cover in the usual way. The new features of this still is the condensing and receiving system, the condensation being drawn into an air-cooled condenser consisting of a single pipe. This pipe leads into a steam-jacketed, three-way valve, one of the valve openings attached to the condensing pipe, and each of the other two leads to a receiver. The steam in the jacket prevents the material from solidifying in the

valve, the material on entering the receiver passing between two glasses. By this means the operator can easily determine when it is shut off, and change from one receiver to another. These receivers are rectangular cast-iron boxes with a door opening the full size at one side. Inside of the receiver is placed a steel box with tapered sides which fits as neatly as possible the full size of the receiver. The Beta Naphthol solidifies in this box which is easily removed and is dumped by taking out the tapered box. In addition to the foregoing, the still is supplied with a large tank in which are placed baffles to collect any material that may pass a receiver. Beyond this tank two smaller receivers are supplied, one for containing the liquid to neutralize the vapors if ány should pass this distance from the still. The other receiver is for this liquor in the event that the air current is reversed in the pipe. This allows the liquor to flow from one tank to the other as the case may be. The entire apparatus is evacuated by means of a dry vacuum pump, and the still is heated by a direct fire.

Another of our developments in the manufacture of Sulfanilic and Naphtionic Acid and for reclaiming high boiling point solvents from waste, is a direct heated shelf retort, consisting of a chamber, provided with ducts passing from one side through an opening on the opposite side. Between the ducts, and inside of the apparatus, are formed shelves. The one side of this retort is provided with a door opening the full size of same. The retort can be operated either with or without a vacuum. The retort when operated under vacuum is connected with a dry vacuum pump and a condensing system between the pump and retort. The material is placed on the shelves in pans and the hot gases from the furnace, in which the chamber is placed, passed through the inside of the shelves. The regulation of the temperature on each shelf is made by dampers which permit more or less of the hot air to pass through the shelf. This type of retort is far more efficient and has a much larger capacity than the previously odd-shaped affairs used for this purpose, and is much more rapid in its operation.

Another improvement that we have made is in the construction of autoclaves for the production of Dimethylaniline. It has always been the practice to use an enamel or silica lining in the autoclave, which has caused more or less trouble due to the checking and cracking and final deterioration of the enamel. This led us to develop an autoclave which uses as a liner a highly acid resistant

cast iron. The difficulty encountered in this construction was that the acid resisting metal is more or less brittle and would not stand for the high pressure the autoclave works under in the manufacture of Dimethylaniline. This led to our adopting the method of backing up the acid resisting castings with a lead lining.

The rapid expansion of the chemical industry has brought up seriously the question of economical acid recovery plants. In this line one of the ingenious and unique developments has been the patented denitrating apparatus, which has for its purpose the distillation of Nitric Acid from a mixture of Nitric, Sulphuric and water. This particular form of construction of the machine enables this nitric to be distilled off at a high strength and reasonably free from lower oxides of nitrogen.

The reasons why results can be obtained from this apparatus and not from an ordinary plain still, are as follows:

The new type of denitrating apparatus presents a very large heating surface in contact with the acid mixture to be treated. The temperature of that part of the apparatus above the level of the acid mixture, cannot by any possibility rise above the temperature of the nitric acid vapors produced in the apparatus. By this means the strongest nitric acid vapors are not decomposed by coming in contact with superheated metal surfaces. Therefore, the very strongest acid, and fairly free from lower oxides of nitrogen, will result.

The distillation of the nitric acid is effected at a much lower temperature than is ordinarily possible. This is done by the injection of air at the bottom of the machine, which keeps the acid mixtures in violent agitation, and the nitric acid held in the mixture is removed most effectively by the sweeping effect of the air. The direct heat from the furnace is drawn through the double U pipes in the apparatus, which are always covered with acid. By this means the greater part of the heat is directly absorbed and the balance of the heat contained in the flue gases after leaving the double U pipes is drawn around, thus giving additional heating surface and giving the greatest degree of efficiency in operation. The distillation of the nitric acid is therefore effected under almost ideal conditions, and by the use of the air the factor of mass action (which has to be considered in an ordinary still when nearly all the acid is distilled off) does not occur in the new type of apparatus. By this I mean that when the ratio of sulphuric acid

to nitric acid toward the end of a distillation becomes so great that a very high temperature has to be carried in the still, the nitric evolved must necessarily be weak.

For example, in handling a Guncotton displacement system the first run-off of spent acid is, of course, reinforced with nitric and oleum, but the second displacement, which consists of approximately 18 per cent nitric, 60 per cent sulphuric and 22 per cent water, if placed in the ordinary still will only give a nitric acid on distillation of about 78 per cent to 80 per cent NNO_3, and even at that the temperature has to be carried to about 180 to 190 degrees Centigrade, and in a fraction of the time taken by the ordinary methods.

It is well known that from Guncotton spent acid, and particularly where oleum is used in making acid mixtures, enormous quantities of sediment will fall down to the bottom of an ordinary still, which is difficult to handle, as it tends to choke the whole system. Now in the new form of denitrating machine, after six months' work, there is not the slightest indication of sediment forming in the machine, and this can be readily understood, as the acid is kept in violent agitation during the whole time of distillation, which prevents the settling of the mud which occurs in the ordinary still.

As to the kind of acid mixture best suited for this new type of machine, would state that any acid mixture containing a nitric content above 2 per cent can be readily handled, and the nitrous contained in the acid mixture is to some extent oxidized by the large volume of air passing into the machine.

As air is used in this machine, which, of course, means that it is saturated with nitric acid to an extent determined by the temperature at which the air vapors pass through the condensers, some form of tower is absolutely essential in order to deprive the air of the nitric acid. The best form of tower for this purpose is a small one through which a very small quantity of water is allowed to trickle so that a fairly strong acid can be obtained at the foot of the tower, and no nitric acid allowed to escape at the top. An ordinary form of absorption tower can be used for this purpose and need only be quite small, 5 feet high by 18 inches in diameter is quite sufficient.

As to the commercial value of this apparatus, as it refers to the handling of waste acid, one may sum this by the following:

1. A stronger and higher grade of nitric acid can be obtained by the new machine than by any of the older processes.

2. The fuel efficiency per unit of nitric obtained is exceedingly high.

3. The capacity of the new machine per unit weight of acid handled is very much higher than with the old type of still.

4. The space occupied by the new apparatus is only one-sixth that required by the older type of still.

5. The life of the new type of apparatus is infinitely greater than the old type of still, as with the new form of apparatus only strong acids come in contact with the metal, and that at lower temperatures.

Some improvement has also been made in evaporators for the chemical and other fields, which has been of great advantage in the development of chemical plants, which started with a small capacity and later desired to increase their production.

The "Buflovak" evaporator has the advantage that it can be doubled in capacity by merely inserting an additional belt and new tubes. This takes up very little additional floor space and gives 100 per cent more capacity.

Another development, which has met with considerable favor in chemical plants, has been the adaptation of the "Butlokast" Crystallizing pan to many uses in the drying and crystallizing of products which tend to be sticky or gummy in the drying process. This crystallizing pan has been very efficient for concentrating and crystallizing material, permitting high temperatures. It has been used heretofore almost exclusively in this country for evaporating and neutralizing liquor of ammonia and nitric acid, and drying and crystallizing the finished product. This pan has also been equipped to operate under vacuum, which provides a means for the drying of sticky materials at low temperatures.

A recent development in equipment for handling acids is the so-called acid-proof or acid-resisting castings, which were first made in England, and later in the United States. We have kept pace with this movement and have manufactured not only small pipes as a substitute for stoneware for conducting acids, but have made various shapes and sizes of retorts. It is only recently that medium-sized castings have been made with this metal. On account of the brittleness of the metal the problem from a foundry standpoint is a very difficult one, but advancement has been made,

both in the size of the castings and in the shapes of them for acid work. No doubt the next two or three years will see a considerably greater improvement in the quality and size of this equipment. I believe that as large and difficult castings are now being made in this county, if not more difficult ones, than in Europe. The use of castings made of this metal is bound to greatly increase on account of such castings being superior to enameled equipment and stoneware for many classes of work.

The Buffalo Foundry & Machine Company has for some years had a chemical laboratory and a physical laboratory for work required in the carrying on of its business. We are now erecting a large laboratory building and will add very materially to the equipment already in use, in order not only to conduct many tests in our experimental dryers and evaporators, but also for research work in organic chemical lines. We expect that there will be a demand for new apparatus to handle the enlarging field of chemical products, and we hope to be able to serve in this development by having a properly equipped laboratory and competent engineering talent to carry on some of the research work.

However, as the old proverb says "Necessity is the Mother of invention," it is important, first, to know what is required before apparatus can be designed for meeting new requirements. In the manufacture of chemicals, it is the man in charge of the plant who meets these new problems and who can call upon the research and designing engineer for his services in solving them. It is, therefore, important that the chemists in charge of the manufacture of chemical products co-operate with the machinery manufacturers in speeding the advance in the design of machines to meet new conditions. It is true that great secrecy surrounds many of the chemical manufacturing processes in this country, and where publicity would affect the earnings of chemical companies it is wise to avoid undue publicity. But giving due regard to the protecting measures required, I believe it is nevertheless possible for manufacturing chemists to submit many problems to the manufacturers of chemical equipment, which the latter can solve to the benefit of both sides of the industry.

DISCUSSION.

The CHAIRMAN: You have heard this very instructive paper. Is there any discussion, or any questions to be asked?

JEROME ALEXANDER: I would ask Mr. Miles if he has any details of the cost of drying on the rotary drum dryer? I believe he said that it was .1 cent of a pound.

H. D. MILES: Do you mean the drum dryer?

JEROME ALEXANDER: The vacuum dryer.

H. D. MILES: The cost for drying varies with different materials.

JEROME ALEXANDER: What have you figured as the cost of fuel?

H. D. MILES: That I have not given.

JEROME ALEXANDER: Is there any charge made in your figures for fuel?

H. D. MILES: Yes, I so stated—the cost of .1 cent per pound covers all expenses including fuel, depreciation, labor and overhead expenses.

JEROME ALEXANDER: That was one of the least costly products to dry. What I want to get at is, what is the average cost of various products in the dryer?

H. D. MILES: It is difficult to establish an average cost. The expense for drying is primarily affected by the amount of output which can be obtained. There is a great variation in the tonnage produced on different materials. For instance, on our standard 5x12 machine an output of one thousand pounds of dry product per hour can be obtained for sulphite waste and some tanning extracts; milk, four hundred pounds per hour, and licorice between five and six hundred pounds per hour. The steam consumption used in drying varies with the amount of moisture driven off, and within reasonable limits the consumption of steam is in proportion to the amount of dry product obtained where the percentage of moisture in the different wet products is approximately the same. The amount of power required for operating the drum varies with the ease with which the material can be moved from the drum. The water consumption in the condenser is affected by the amount of vapor being handled. The other charges, such as labor, overhead burdens, depreciation, etc., are practically a constant factor irrespective of the amount of product delivered by the machine.

JAMES WITHROW: What use do the sulphite manufacturers make of the dried product?

H. D. MILES: It is used as a binder in core compounds for foundry purposes, for road dressing, as a leather filler by tanners, and for other purposes with which I am not familiar. The chemist who has done most to develop its use claims that he has discovered two hundred and fifty uses for it. He may be too much of an optimist, but it is a fact that dry sulphite waste is growing in favor, and the uses for it are rapidly developing. We have, in the last two years, sold two of our largest drum dryers to one company for drying sulphite waste.

THE CHAIRMAN: Is there any further discussion?

E. J. K. MASON: I would inquire of Mr. Miles whether any work has been done by his firm along the lines of electric heaters for vacuum dryers. He spoke of the use of steam, etc.

H. D. MILES: We have never tried electric heat except experimentally with a vacuum drying and impregnating machine, which is used for impregnating electric coils for street railway and other motors, as it was thought that such a method might be important where steam was not available, but we never got beyond the experimental state.

E. J. K. MASON: Can satisfactory results be obtained by the use of steam for heating in the vacuum dryers?

H. D. MILES: That depends again on the product. Steam is generally used in the different types of vacuum drying apparatus, but, where materials to be dried are very sensitive to heat, it is sometimes necessary to use hot water instead of steam. By varying the temperature of the hot water, about any degree of heat can be obtained between the limits of 212 and atmospheric temperature. The output of the dryer, however, is reduced in proportion to the reduction in temperature.

THE EFFECT OF CENTRIFUGAL FORCE ON COLLOIDAL SOLUTIONS

By EUGENE E. AYRES, Jr.

Read at the Ninth Annual Meeting, New York City, Jan. 12, 1917

The settling of small particles in a liquid is a matter of common observation. The rate of such settling depends, of course, upon the size and weight of the particles and the viscosity of the liquid. There are many processes for increasing the size of the particles, such as the addition of electrolytes to cause coagulation, but the weight of the particles can be increased, independently of their mass, only by substituting a force greater than the gravitational attraction of the earth. Centrifugal force can be generated to a degree many times the acceleration of gravity.

Up to a very recent date the highest centrifugal force applied to industrial separations was about four thousand times gravity. To-day a force ten times as great is commercially developed, and interesting questions have arisen with reference to the types of separations that can be accomplished. It is only natural that considerable work should have been done on certain centrifugal separations which cannot be accomplished by the highest degree of centrifugal force at present capable of commercial application. Some chemists have expressed surprise that gasoline cannot be separated from kerosene, or that soap cannot be mechanically removed from aqueous solution. Others are inclined to doubt that colloidal gold can be successfully deposited from a clear gold sol.

There is one certain method of eliminating guesswork as to the practicability for a given set of conditions—and that is the experimental method. If a careful trial succeeds or fails, the practicability for that set of conditions is no longer a matter of opinion. But pertinent questions may still be asked. For instance—Will the result secured by the test represent the best that any type of centrifugal machine may be expected to secure? Is it possible that a higher degree of force can actually remove anything unaffected by the lower degrees? Is it possible that, although complete

separation may have been secured, the components are not separately discharged? Such questions can be correctly answered only by thoroughly understanding the complexity of the task confronting the centrifugal separator.

The Colloid State.—Let us first review briefly a few general aspects of colloid chemistry.

The term *dispersoid* has been correctly applied to systems involving every size of particle, from the macroheterogeneous suspension to the true molecular or ionic solution. The term *colloid* in its exact sense designates a state of matter with respect to degree of dispersity within the rather narrow limits defined by submicro- and amicro-heterogeneity. The largest colloid particles in this sense are about 0.1μ[1] in diameter—at the extreme limit of microscopic visibility, and at about the limit of retention by the best clay filters.[2] Particles smaller than 0.1μ may be seen by use of the ultramicroscope, whose lower limit is about $3\mu\mu$. Colloidal phenomena are exhibited by dispersoids whose particles are anywhere from 0.1μ to $1\mu\mu$. Most solutions, or molecular dispersoids, are just beyond this limit, a fair average molecular diameter being about $0.1\mu\mu$, although some of the more complex molecules are much larger.[3]

All these systems are polyphasic, but the disperse phase will confer various sets of properties upon the dispersoid, depending upon the specific surface. Therefore the complex system involving many different degrees of dispersion may be said to have distinct phases corresponding to the three series:

1. Particles larger than 0.1μ.
2. Particles between 0.1μ and $1\mu\mu$.
3. Molecules.

The second division defines the colloid state in its strictest sense. But many of the phenomena that characterize the true colloid characterize to a greater or less degree the coarser dispersoids, whose particles may be even larger than 5μ, and can be retained by the ordinary filter paper.[4]

[1] The μ, or micron, is 0.0001 cm. The $\mu\mu$ is 0.001μ or 0.0000001 cm.

[2] Wo. Ostwald-Fischer, "Handbook of Colloid Chemistry," p. 263.

[3] The diameter of the starch molecule is said to be about $5\mu\mu$. Lobry de Bruyn and Wolff, Rec. Trav. Chim. Pays-Bas., 23, 155 (1904).

[4] Ordinary filter paper holds back particles of about 5μ; a hardened filter (Schleicher and Schull, No. 602 e.h.), those about 2μ in diameter. (Wo. Ostwald, *l. c.*).

Certain classifications of dispersoids have been made without regard to degree of dispersion. It will be unnecessary to discuss these distinctions here except to mention the colloids that tend to form a gel. There are several characteristics of these gel-forming dispersoids, especially affecting centrifugal separation. The particles are usually extremely small—even beyond the limit of the ultramicroscope—and the disperse phase and the dispersion medium are composed of both components.[1] The solid gel has an enormous viscosity factor. This would prevent any centrifugal separation even under the most favorable conditions. When the gel is warmed, to liquefy and reduce viscosity, the size of the particle is even further reduced, and the compositions of the two phases tend more nearly to equalize in percentage of components. When this equilibrium is approximately established, the absolute density of the disperse phase is therefore nearly zero.

We shall put aside the gel-forming colloid for the simpler case of the suspensoid whose particles, whether rigid or deformable, have little chemical or solvent affinity for the dispersion medium.

At first glance, it might appear necessary to limit our problem still further. In the case of separation by gravity, the degree of dispersion is likely to change enormously on account of some of the influences commonly known to affect stability. Since time, in the case of centrifugal separation, is a matter of minutes rather than days, we can eliminate these factors.

A Few References.—With reference to the theme of this paper —the study of colloids under the influence of centrifugal force— the first task will be to formulate a mathematical expression of the phenomenon of subsidence. The subject has already been treated in a variety of ways.

The probable influence of gravity on solutions was studied by Gay Lussac. He examined the upper and lower portions of a salt solution in a tube two meters long maintained at a constant temperature for a long time. The results were negative. In 1887, Gouy and de Chaperon, commenting on this work, demonstrated

[1] Hardy, Proc. Roy. Soc., 1900, 66, 95. Agar gel (2.23 per cent).

	Concentration Agar.	
Temp.	Liquid.	Solid.
36°	0.47	.2
5°	0.09	3.0

theoretically that the force of gravity was too small. L. Vegard [1] from thermodynamical considerations, gives an expression for the equilibrium of a solute. H. S. Davis [2] gives a mathematical analysis of the effect of gravity on a solute, by applying Fourier's law of linear diffusion. His experimental results are also negative.

One of the earliest of the difficult centrifugal separations was made as long ago as 1895 on a mixture of two gases—hydrogen and hydriodic acid.[3] The gases were whirled for three hours in a machine operated at 2400 r.p.m. with a radius of rotation of 21 centimeters. A 3 per cent difference in concentration was noted.

Quite recently, Professor Schaefer of the Technical School at Charlottenburg is said to have passed air through a centrifuge operating at 1200 to 2000 r.p.m., with a resulting concentration of oxygen from about 21 per cent to 26 per cent. The writer is not prepared to verify this reference.

About twenty years ago, it was observed that subjecting a salt solution to moderate centrifugal force will cause a measurable difference of potential between the center and the periphery. More recently [4] a centrifuge was successfully employed to obtain a concentration of antitoxic substances in immunitive serums. Perhaps the most striking work on the centrifugation of solutions was done by Van Calcar and Lobry de Bruyn[5] who were able to crystallize out about three-eighths of the salt from a saturated Glauber's salt solution.

The Problem.—When any dispersoid is subjected to centrifugal force—no matter in what type of apparatus—the separation of any individual particle will depend upon the distance through which it must move in a given time before being considered " separated." There are two types of centrifugal machines. In one case, a container is filled with liquid and rotated. The particle must move until it is held in firm contact with the bottom of the vessel before

[1] Contributions to the Theory of Solutions. (Phil. Mag., Series 6, No. 77, p. 258).

[2] Transactions of the Nova Scotia Inst. of Science. Vol. XII, Part 3, pp. 291–301, 1912.

[3] Bredig, Zeitsch. f. physik. Chem., 17, 459.

[4] R. P. Van Calcar, Voir, Verslagen, March 19, 1904, p. 842.

[5] Rec. Trav. Chim. Pays-Bas., Vol. 23, pp. 218–223, 1904.

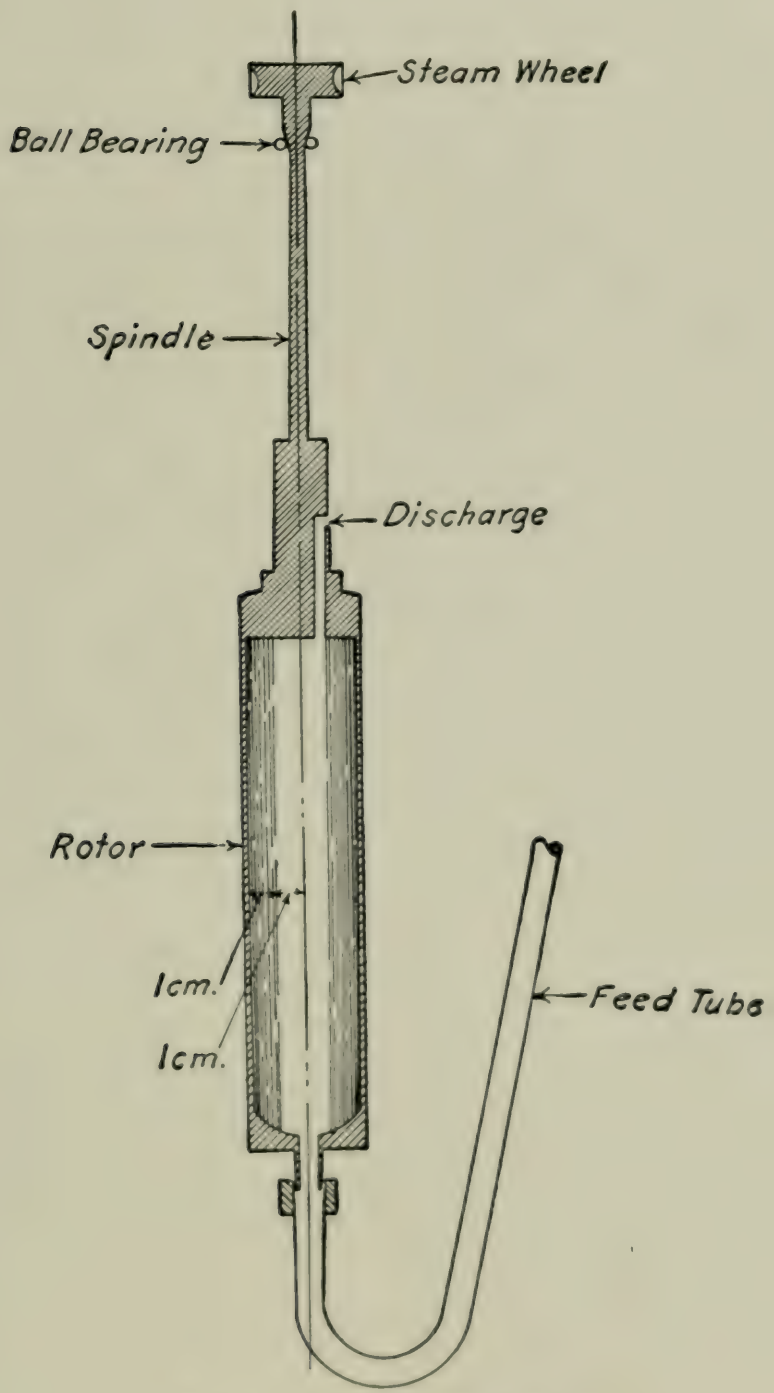


FIG. 1.—Continuous Centrifuge.

any separation can be measured. In the other case, the liquid is passed continuously through a rotating cylinder and it is possible to draw the liquid from the point nearest the center of rotation while the machine is still in motion. Separation can therefore be detected when the particle has been moved only a fraction of the distance from the surface to the bottom. Our consideration will be limited to this more convenient type.

It would seem that the whole problem hinges upon the proper calculation of the velocity of a suspended particle acted upon by a force. But what is the force? When we think of velocity, we naturally turn to the law of Stokes.

$$U = \frac{f}{6\pi r\eta},$$

where U is velocity of the particle of radius, r, acted upon by the force f, while the viscosity of the medium is η. The formula is based upon the conception that a small body, acted upon by a constant force, will move through a resisting medium with a constant velocity. The normal acceleration is neutralized by friction. The law was applied first to the fall of raindrops through the air. More recently, a large number of important investigations have been based on Stokes' equation, and its accuracy has been verified for liquid as well as gaseous media, and for colloidal particles as well as raindrops.

J. Perrin [1] showed the law to be valid for particles as small as one-tenth of a micron. Einstein,[2] in his work on the coefficient of diffusion, assumed Stokes' law to be correct for molecules larger than one-thousandth of a micron, and his results show his assumption to be correct. On the other hand, M. Pellat,[3] in his investigation of ionic translation in electrolysis, gives some indication that Stokes' law will require a slight modification when applied to particles as small as the potassium ion. Of course our chief interest is in the relatively large particles to which the law has been found to apply without correction.

We are told, then, that colloids will settle by gravity according to a given formula. Emil Hatschek [4] offers a numerical example in

[1] "Brownian Movement and Molecular Reality."
[2] Ann. der Physik., 1906, XIX, 289.
[3] Traité d'Electricité, Vol. III, p. 56.
[4] "Introduction to the Physics and Chemistry of Colloids," p. 24.

the case of a gold sol, and utilizes the very evidently incorrect result as an interesting clue to certain other factors that make for the stability of colloids.

What are these other factors? It is true that if precautions are taken, as recommended by Perrin, Wo. Ostwald, and others, to avoid small convection currents by observing the rate of fall in a capillary tube, the initial rate of fall will correspond to the equation But after a time the rate will retard and then cease altogether. In other words, the force of gravitation has been neutralized by other forces acting upward, and the system is at equilibrium.

The Dynamics of Colloids.—Just as Van 't Hoff has been the pioneer in developing the analogy of dilute molecular dispersoids to gases, so M. Jean Perrin has done the first work in extending the kinetic theory to colloidal and even subcolloidal degrees of dis-

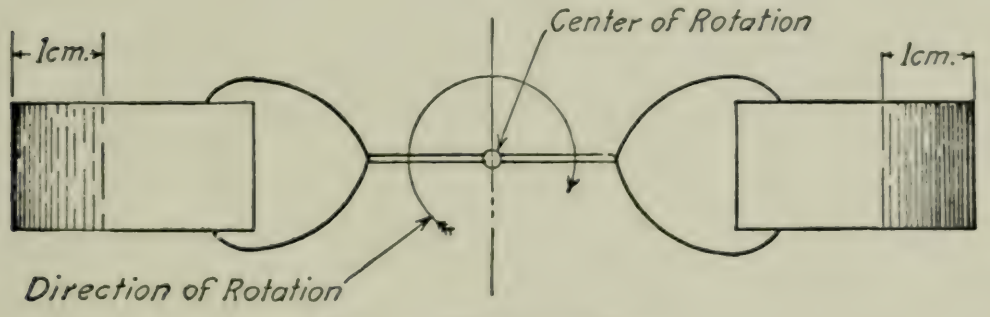


FIG. 2.—Tube Centrifuge.

persion. Van 't Hoff had shown that the molecules of a solute in dilute solution play the same part as the molecules of a gas—develop the same kinetic energy, and exert a similar pressure on the walls of a semi-permeable membrane.

The earlier work on the osmotic pressure of colloids indicated a small but invariable value—sometimes so small as to raise a suspicion of experimental error or impure sols, but so invariable as to lead finally to the conclusion that all colloids did exert a definite osmotic pressure. Perrin showed that the osmotic pressure of colloids is due to their own kinetic energy, an energy derived from the molecular energy of the dispersion medium by molecular impacts on the colloidal particle. Kinetic energy implies motion, and colloids are found to possess an irregular, translatory motion—Brownian movement, from the name of its discoverer.

Osmotic pressure is thus a property common to all dispersoids

whose particles are small enough to show Brownian movement. The largest particles showing this movement are about 10μ in diameter—just about visible to the unassisted eye—but their osmotic pressure is negligibly small. In fact, some particles will settle completely by gravity when less than one micron in diameter.

A second property of some dispersoids is an electric charge. The charge may be either positive or negative, and may be caused either by adsorption of electrolytes or by an actual ionization of the particles. Naturally, the presence of equal electric charges of similar sense would seem to lead to an electro-repulsion between particles.

Osmotic pressure can be easily measured in a given case by the usual methods of physical chemistry. The size and sense of the electric charge can be measured by the speed of migration in an electric field.[1] This has been done and the conclusion was drawn that the electro-repulsion cannot be great enough to affect subsidence, although it greatly influences the rate of what is termed "agglomeration crystallization." So there remains only the osmotic pressure.

Instead of measuring this pressure and in some way computing its upward component, it is more convenient to start with the law of distribution derived for colloids by J. Perrin.

The Law of Distribution.—Imagine a liquid containing a number of small particles of uniform size and distributed uniformly throughout the liquid. After the system has been allowed to stand for a time, the particles will have arranged themselves in a definite order. The concentration (number of particles per unit volume) will be greatest at the bottom, and will vary in an exponential manner with the height according to the equation:

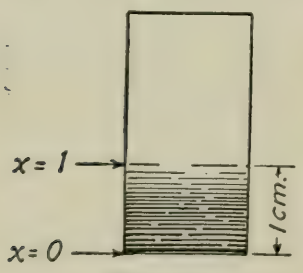


FIG. 3.
Gravity Subsidence.

$$\log_e \frac{c_0}{c_x} = mgkx,$$

where c_0 is the concentration of particles at the initial level, o, (see Fig. 3), c_x is the concentration at the height x, m is the mass of the particle, g is the acceleration of gravity, k is a constant.

If we put

$$mgk = \alpha,$$

[1] The Svedberg, Koll.-Zeitschr., Vol. VI, 238 (1910).

we may write the expression

$$c_x = c_0 \cdot e^{-\alpha x},$$

In order to determine the concentration at any point, x, we assume an empirical initial concentration—in other words, we take a value, k_1, to represent the number of particles in a unit volume when the system was uniform.

We can now write

$$k_1 = \int c_x dx = \frac{c_0}{-\alpha} \cdot e^{-\alpha} + c'.$$

The depth of liquid is conveniently taken as 1 centimeter. We can therefore integrate between the limits, 1 and 0.

$$k_1 = \int_0^1 c_x dx = \frac{c_0}{-\alpha}[e^{-\alpha} - 1],$$

whence

$$c_0 = k_1\left(\frac{\alpha}{1 - e^{-\alpha}}\right),$$

and

$$c_x = k_1\left(\frac{\alpha}{e^{\alpha x} - e^{\alpha(x-1)}}\right).$$

By the substitution of any value of x between 1 and 0 we can find the concentration at x as a function of k_1 and α.

You will remember that

$$\alpha = mgk.$$

k is a constant equal to Avogadro's constant divided by the product of the perfect gas constant and the absolute temperature. Assuming a normal temperature, the constant k is about equal to 3×10^{13}, in c.g.s. units. The mass, m, is equal to the volume times the absolute density, d.

$$m = \tfrac{4}{3}\pi r^3 d;$$

$$\alpha = \tfrac{4}{3}\pi r^3 d \cdot 980 \cdot 3 \cdot 10^{13};$$

$$\alpha = 10^{17} \cdot r^3 d.$$

For particles of different sizes and densities, α can be easily computed and substituted in the formula for concentration. from

which the condition at equilibrium can be shown for any value of x. It is more convenient to express k_1, as a weight percentage.

The following tabulation will give a fair idea of the various arrangements with different values of α.

α	c_1	$c_{.5}$	c_0
1	0.58	1.04	1.60
10	0.001	0.10	10.00
100	10^{-40}	10^{-20}	100.00

In each case, the initial concentration, k_1, was taken as 1 per cent.

Similar values can be easily computed for any other values of x or for smaller or larger values of α, or for other values of k_1.

It is seen that the arrangement at equilibrium is a function of α. But α is the product

$$10^{17} r^3 d$$

so that the radius of the particle that will give a certain α will depend upon the absolute density, d. Thus, when $\alpha = 1$, a particle of low density must have a radius of 10^{-5} centimeter, whereas if the density is high, the radius must be 10^{-6}.

The condition represented by $\alpha = 100$ leaves no room for doubt. Gravity separation will be complete. In such a mathematical application, the liquid can never have a concentration at any point equal to absolute zero, because in such a case the equation for distribution would be indeterminate. But such concentrations as, for instance, 10^{-20} per cent, can be considered as *practical* zero. The condition represented by $\alpha = 10$, also means a very thorough separation. When $\alpha = 1$, the separation, though not at all complete, is still easily measurable.

It is interesting to note that Zsigmondy found the gravity subsidence of a gold sol to be measurable when the particles were about $75\mu\mu$ in diameter. Such a particle with the high density of gold would make α equal to about 1. Occasionally we come across statements that particles usually will not settle by gravity when as small as 0.1μ. Theoretically the condition of equilibrium shown for $\alpha = 1$ should obtain unless the electro-motive forces should

bring about agglomeration crystallization. In this case, subsidence will be complete.

We might stop to compute the effect of gravity on the distribution of true solutions where α is very small, but we must go on to apply the formulæ to centrifugal machines.

If it were possible to construct a centrifuge with an infinitely long radius of rotation, we could use precisely the same formulæ for centrifugal separation as for gravity subsidence. But, given a constant r.p.m., the force varies directly with the radius of rotation, and the force is greater at the periphery, o, than at the surface, 1. In Fig. 4, let the distance from the center of rotation to the periphery be designated by R. The radius of rotation of any point, x, may be expressed by $R-x$. Therefore we have

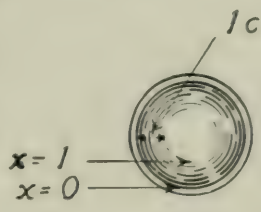


FIG. 4.—Cross-section of Continuous Centrifuge.

$$\frac{f_0}{f_x}=\frac{R}{R-x},$$

$$f_x=f_0\frac{R-x}{R},$$

where f is the factor multiplying the acceleration of gravity. The centrifugal force, $f_x g$, at any point, x, is equal to a function of x involving the centrifugal force, $f_0 g$, at the periphery.

The general expression for distribution in the case of centrifugal force is

$$\log_e\frac{c_0}{c_x}=mf_xgkx,$$

or

$$=mkf_0gx\left(\frac{R-x}{R}\right).$$

Let $\beta=mkf_0g$,

and let $k_1=\int c_x dx=c_0\int e^{-\beta x\left(1-\frac{x}{R}\right)}dx.$

This function cannot be actually integrated by the ordinary calculus. But approximation by the prismoidal form yields a very nearly accurate result between o and 1.

$$k_1=\frac{c_0}{6}\left[1+4e^{-.5\beta\left(1-\frac{.5}{R}\right)}+e^{-\beta\left(1-\frac{1}{R}\right)}\right].$$

In the special case of the centrifuge represented diagrammatically in Figs. 1 and 4, the value of R is 2, and the depth of liquid is 1 centimeter. The expression therefore becomes

$$k_1 = \frac{c_0}{6}[1 + 4e^{-.375\beta} + e^{-.5\beta}];$$

$$c_0 = k_1\left[\frac{6}{1 + 4e^{-.375\beta} + e^{-.5\beta}}\right];$$

$$c_x = c_0\left[e^{\beta x}\left(\frac{x}{2} - 1\right)\right]$$

$$c_x = k_1\left[\frac{6e^{\beta x}\left(\frac{x}{2} - 1\right)}{1 + 4e^{-.375\beta} + e^{-.5\beta}}\right].$$

This equation is identical in purpose with the simpler form derived for gravity subsidence. k_1 represents concentration throughout the liquid before subsidence. It is seen that the factor essentially influencing centrifugal subsidence is

$$\beta = mkf_0g.$$

In the case of the particular centrifuge in mind, f_0 is 40000. Therefore

$$\beta = 4 \times 10^{21} r^3 d.$$

Let us see what happens when we subject a 1 per cent dispersoid to such a force.

β	c_1	$c_{.5}$	c_0
1	0.80	0.95	1.36
10	0.06	0.08	6.00
100	10^{-20}	10^{-10}	6.00

The results for equilibrium under centrifugal force are seen to be very similar to those for gravity subsidence. Here again we find that separation is good when $\beta = 10$ and practically perfect when $\beta = 100$.

It should be clearly understood that the formulæ given above are expressions of equilibrium. If a dispersoid whose particles are just visible to the ultra-microscope is subjected to a force of 40,000

times gravity, the particles will be thrown down, *provided the force is applied for a sufficient time.*

Computation of Time.—Many observations in the laboratory under widely varying conditions have led the writer to conclude that precisely the same time is required for each particle of a given dispersoid to move from its position of uniform distribution to the positions of equilibrium. But that this assumption must be correct is evident, not only from observation but from a study of the principles underlying the law of distribution. The particle at $x = 1$ will move with greater velocity and a longer distance than the particle at $x = 0.5$, but the time will be identical.

The centrifugal force at any point is

$$f_x g = f_0 g\left(1 - \frac{x}{2}\right).$$

The centrifugal force acting upon the particles represented by the concentration, k_1, can be expressed by

$$f_x g = k_1 f_0 g\left(1 - \frac{x}{2}\right).$$

Earlier in this paper, it was pointed out that the only considerable force opposing gravity is osmotic pressure.

The osmotic pressure of one particle in unit volume, according to the kinetic theory, is equal to the reciprocal of the constant, k, that enters into our functions of α and β. When c_x represents a concentration by weight percentage, the number of particles in unit volume is

$$\frac{c_x}{m},$$

where m is the mass of one particle. The number of particles in the volume $(1-x)$ is

$$\frac{c_x}{m}(1-x).$$

The osmotic pressure, P, at any point, x, is therefore

$$P_x = \frac{c_x}{mk}(1-x).$$

Now

$$c_x = k_1\left[\frac{6e^{\beta x}\left(\frac{x}{2}-1\right)}{1+4e^{-.875\beta}+e^{-5\beta}}\right],$$

and if we let

$$\phi(\beta)=\frac{1+4e^{-.375\beta}+e^{-.5\beta}}{6},$$

we can write

$$P_x=k_1\frac{e^{\beta x\left(\frac{x}{2}-1\right)}}{mk\phi(\beta)}(1-x),$$

and since

$$mk=\frac{f_0 g}{\beta},$$

we can write

$$P_x=f_0 g\cdot k_1\frac{e^{\beta x\left(\frac{x}{2}-1\right)}}{\beta\phi(\beta)}(1-x).$$

The actual force F_x exerted on the particles of a dispersoid can therefore be expressed by

$$F_x=k_1 f_0 g\left[1-\frac{x}{2}-(1-x)\frac{e^{\beta x\left(\frac{x}{2}-1\right)}}{\beta\phi(\beta)}\right].$$

When Stokes' law is applied to particles of given mass acted upon by the acceleration of gravity, it is necessary to put

$$f=mg$$
$$=\tfrac{4}{3}\pi r^3\cdot d\cdot g.$$

Thus, we have

$$U=\frac{2r^2dg}{9\eta},$$

or for the special case of the centrifuge

$$U_x=\frac{2r^2dF_x}{9\eta},$$

$$U_x=\theta(\beta)\left[1-\frac{x}{2}-(1-x)\frac{e^{\beta x\left(\frac{x}{2}-1\right)}}{\beta\phi(\beta)}\right],$$

where

$$\theta(\beta)=k_1 f_0 g\frac{2r^2d}{9\eta}=k_1 f_0 g\frac{\beta}{18kr\eta}.$$

Now velocity, by definition, is the distance traversed in unit time. The time required to traverse the distance $(1-x)$, must be equal to

$$\frac{1-x}{U_x}.$$

$$\text{Time}_{(1-x)} = \frac{1-x}{\theta(\beta)\left[1-\frac{x}{2}-(1-x)\frac{e^{\beta x}\left(\frac{x}{2}-1\right)}{\beta\phi(\beta)}\right]}.$$

But, as we pointed out above, the time is not a function of the distance traversed. It is evident that the distance $(1-x)$ is a function of β, and since the distance to be traversed by any particle depends upon the factor defining the concentration, we may put

$$x=\phi(\beta)=\frac{1+4e^{-.375\beta}+e^{-.5\beta}}{6}.$$

Substituting in the equation for time, we have

$$\text{Time} = \frac{1-\phi(\beta)}{\theta(\beta)\left[1-\frac{\phi(\beta)}{2}-(1-\phi(\beta))\frac{e^{\beta\phi(\beta)}\left(\frac{\phi(\beta)}{2}-1\right)}{\beta\phi(\beta)}\right]}.$$

This expression gives the time required for particles to traverse from positions of uniform distribution to positions of equilibrium, in a centrifuge whose radius is 2 and whose depth of liquid is 1. The value is the same for particles of given mass and for liquids of the same value of viscosity.

The following tabulation shows the time,[1] according to this formula, required for separation of dispersoids whose dispersion media is water ($\eta=0.01$ in c.g.s. units). The density of the particles is taken as 1, and the concentration, k_1, is 1 per cent.

Radius.		β	$\theta(\beta)$	Time.
Cm	μ			
10^{-3}	0.1$\mu\mu$	4×10^{-3}	8×10^{-9}	13 days
10^{-7}	1.0$\mu\mu$	4	8×10^{-6}	33 hours
10^{-6}	10.0$\mu\mu$	4×10^{3}	8×10^{-4}	18 minutes
10^{-5}	0.1μ	4×10^{6}	8×10^{-2}	11 seconds
10^{-4}	1.0μ	4×10^{9}	8×10^{0}	.1 second
10^{-3}	10.0μ	4×10^{12}	$8\times10^{+2}$	.001 second

It will be seen that when β is larger than 100, the time is about equal to $\frac{1}{\theta(\beta)}$ in seconds.

[1] It must be remembered that these figures will apply only to the centrifuge exerting a force of 40,000 times gravity and having a radius of 2 centimeters. For any other force or diameter, the proper expressions can be easily derived from the general forms.

Some objections may be offered to the derivation of our equation for time. Whatever the merits of the case, our equation has two rather unusual characteristics, it is easy to use, and its results, as far as we have observed, will accord with every laboratory measurement.

Practical Considerations.—The great point of superiority of the continuous centrifuge over the non-continuous type is the possibility of handling practical volumes of liquid. When the volume is great, the liquid must pass quickly through the rotor and the time during which the centrifugal force can act on the dispersoid is limited. The time in each case is dependent upon the stationary capacity of the rotor. For instance, if we must pass 10 liters of liquid into a given rotor before the level is high enough to overflow (or discharge), it would seem that the time in minutes during which each unit of liquid remains in the rotor will be equal to the rate of flow in liters per minute divided by 10. As a matter of fact, the actual time will be less than this quotient by an interval depending upon the mechanical arrangements of feeding, discharge, etc. Naturally, for any commercial application, a continuous centrifugal will be practical only when it can handle certain required volumes per day. The sizes of particles removed under such restrictions of time will be definitely limited. The capacity of the high speed centrifugal rotor has been limited in practice by strength of materials, but the degree of centrifugal force exerted has not yet reached any such practical limit. It would therefore seem worth while to answer the question: Will any advantage be gained by applying a much greater centrifugal force?

From the tabulation below, it is easy to see a partial answer. These calculations are based on a viscosity of 1. Values of f_0 are taken from 10^2 to 10^7.

r	10^2	10^3	10^4	4×10^4	10^5	10^6	10^7
100μ	0.5 sec.						
10μ	50 sec.	5 sec.					
1μ	1½ hrs.	8⅓ min.	50 sec.	12 sec.	5 sec.		
0.1μ	.....	.....	1½ hrs.	20 min.	8⅓ min.	50 sec.	5 sec.
10$\mu\mu$	.....	.....	.....	.....	.....	1½ hrs.	8⅓ min.

The values of β for every size of particle with the corresponding centrifugal force shown here are so large that the separation will be complete in every case.

The highest degree of centrifugal force at present commercially applied is 40,000. The lowest practical rate of continuous flow represents in this case a maximum time of about twenty minutes. One per cent suspensions of particles of various densities in liquids of various viscosities can therefore be completely separated by this machine in continuous operation according to the following table:

	Size of Particle.	Viscosity.
$d = 1.00$	$1.0\mu\mu$	.0001
	$3.0\mu\mu$	.001
	$10.0\mu\mu$	.01
	$30.0\mu\mu$	.10
	0.1μ	1.00
	0.3μ	10.00
$d = 0.01$	$10.0\mu\mu$	.0001
	$30.0\mu\mu$	.001
	0.1μ	.01
	0.3μ	.10
	1.0μ	1.00
	3.0μ	10.00

Any continuous flow machine can be operated with small separate lots of liquid as an ordinary "bottle" centrifugal. In this way it is possible to remove even smaller particles than shown above.

The determining factors are seen to be viscosity and absolute density. The minimum size of particle is affected by both to the same extent, for we see from the approximate equation for time,

$$\text{time} = \frac{9\eta}{f_0 g k_1 {}_2 r^2 d},$$

that

$$\frac{r_1^2}{r_2^2} = \frac{d_1}{d_2} = \frac{\eta_2}{\eta_1}.$$

In practice, one will rarely meet with a case where the density of the medium and the density of the particle are the same to the second decimal place. When such an equality is approximated, it is well to bear in mind the fact that density increases with degree of dispersity. For instance, the density of a globule of water $30\mu\mu$

in diameter is 0.5 per cent greater than that of water *en masse*.[1] The density of gold in various forms has been determined[2] as follows:

Molten and compressed	19.33
Colloid, ppt. by ferrous sulphate	20.71

The question has sometimes been raised as to whether viscosity, the measure of the internal friction of a liquid, can be truly the

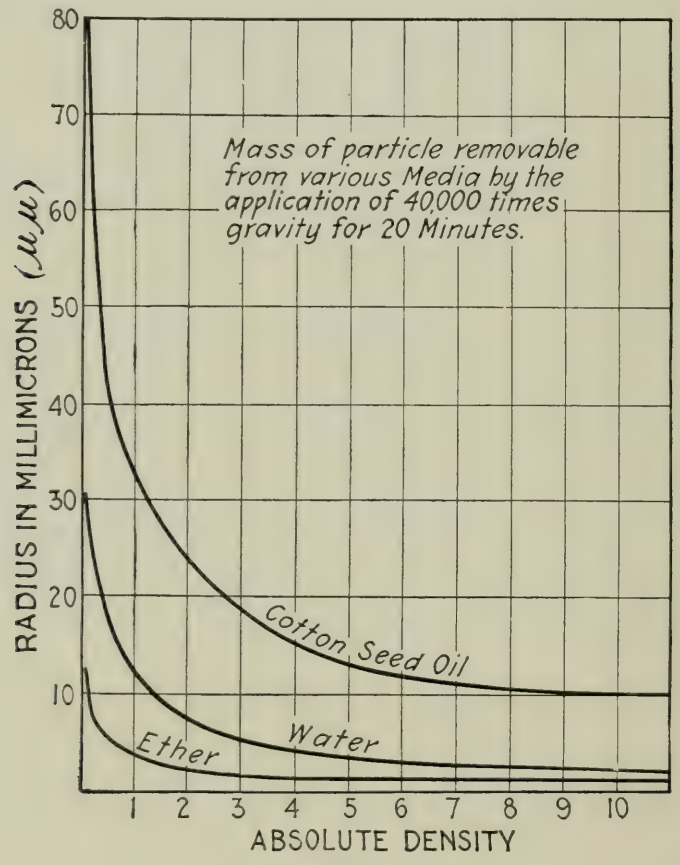


FIG. 5.

absolute measure of the resistance to the motion of a particle. One might expect the physical characteristics of the particle to be a potent factor. But it is believed that each colloid particle is enveloped by a permanent film of the dispersion medium, and that whenever the particle is moved, the film will be dragged along.

The accuracy of the figures given for the high power machine

[1] "Handbook of Colloid Chemistry," Wo. Ostwald-Fischer, p. 121.

[2] G. Rose, Poggendorf's Ann., Vol. LXXIII, 1 (1848).

has been verified in a number of ways by experimental work. In some cases the value of β has been so small that separation could not be complete and the theoretical concentration of the dispersoid was computed from the equation for c_x. In cases where β was large, and the liquid was drawn off before a state of equilibrium (i.e., complete separation) had been reached, the concentration was computed from the approximate formula for velocity. When β

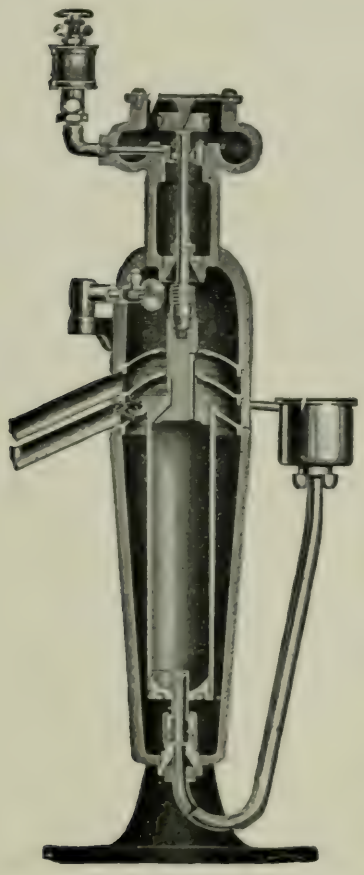


FIG. 6.—Sectional Cut, Continuous Centrifuge.

FIG. 7.—Continuous Centrifuge in Operation.

is large the change in concentration is directly proportional to the time, whereas with a small value of β, the change in concentration will vary as an exponential function of the time. In other words, the time required for the particle to move the first millimeter will be very much less than the time required to move the next millimeter.

One interesting fact, which has been confirmed in practice, is that the particles of a polydispersoid (containing particles of several

degrees of dispersity) can be separated accurately in fractions by applying various degrees of force.

For instance, a serum containing finely divided fibrin and corpuscular debris of various degrees of dispersity was subjected to a force of about 1700 times gravity. After the force had been applied fifteen minutes, the serum was perfectly transparent to the naked eye. But a fine porcelain filter was immediately clogged, showing that many particles larger than one micron still remained. The liquid was then whirled again with the same force for two hours. No sedimentation could be seen, and the Berkfeld filtration was equally difficult. A portion of the original serum was now passed through the centrifuge, exerting 40,000 times gravity, in which the time of application was five minutes. The resulting liquid contained only a few particles as large as 0.5 micron and 50 liters could be passed through the filter before cloggage.

An emulsion of crude oil and water was found by distillation to contain 9 per cent water. When diluted with gasoline and subjected to the force of 1700 times gravity, 0.9 per cent of water was separated in thirty minutes. The percentage recovery was not increased by extending the time to five hours. The same gasoline mixture was passed through the high power centrifugal at a rate that insured less than a minute for separation. The yield of water was 8.8 per cent.

These two examples might be multiplied indefinitely from the writer's experience. They show in a concrete manner that a separation that is impossible to secure by a lower degree of force, even though the time be extended far beyond the limits of practicability, can often be secured in a very short time by a more powerful machine.

The Centrifugal Separation of Solutes.—The experiment of Van Calcar and Lobry de Bruyn, mentioned earlier in this paper, in which three-eighths of the salt was crystallized out from a saturated Glauber's salt solution by whirling in a centrifuge, is very interesting in view of the theoretics developed above. With the comparatively low force applied, and the extremely small size of the particles, the value of β would be very small (about 10^{-4}). The condition of equilibrium of the molecules would therefore show an increase in concentration at the periphery of a very small fraction of a per cent. But this slight increase means *supersaturation*, and supersaturation has been described by P. P. Von Weimarn as a transi-

tion between the true molecular dispersoid and the colloid. The value of β would therefore become correspondingly greater, and toward the end of the period (five hours) the effect of agglomeration crystallization would be quite noticeable. This is, therefore, a somewhat more complex problem than a mere centrifugal separation.

The partial separation of certain gases should, according to our calculations, be within the range of possibility. With 40,000 times gravity, the value of β would be small and separation necessarily incomplete, but the *time* required to reach this state of equilibrium should be reasonably small on account of the very low viscosity. The continuous separation of gases has not yet been actually tried in practice on account of certain mechanical difficulties of separate discharge, but these difficulties will no doubt be overcome. *Complete* separations of gases cannot be accomplished by any force less than about one hundred million times gravity.

FIG. 8.—Commercial Centrifuge for Separation of Liquids.

If a mixture of gasoline and kerosene is passed through the rotor exerting 40,000 times gravity, at the slowest practical rate the difference in concentration between the layers at the surface and at the periphery would be much too small for any analytical detection. If the mixture were held in the rotor for three weeks, an equilibrium would be established, but even then

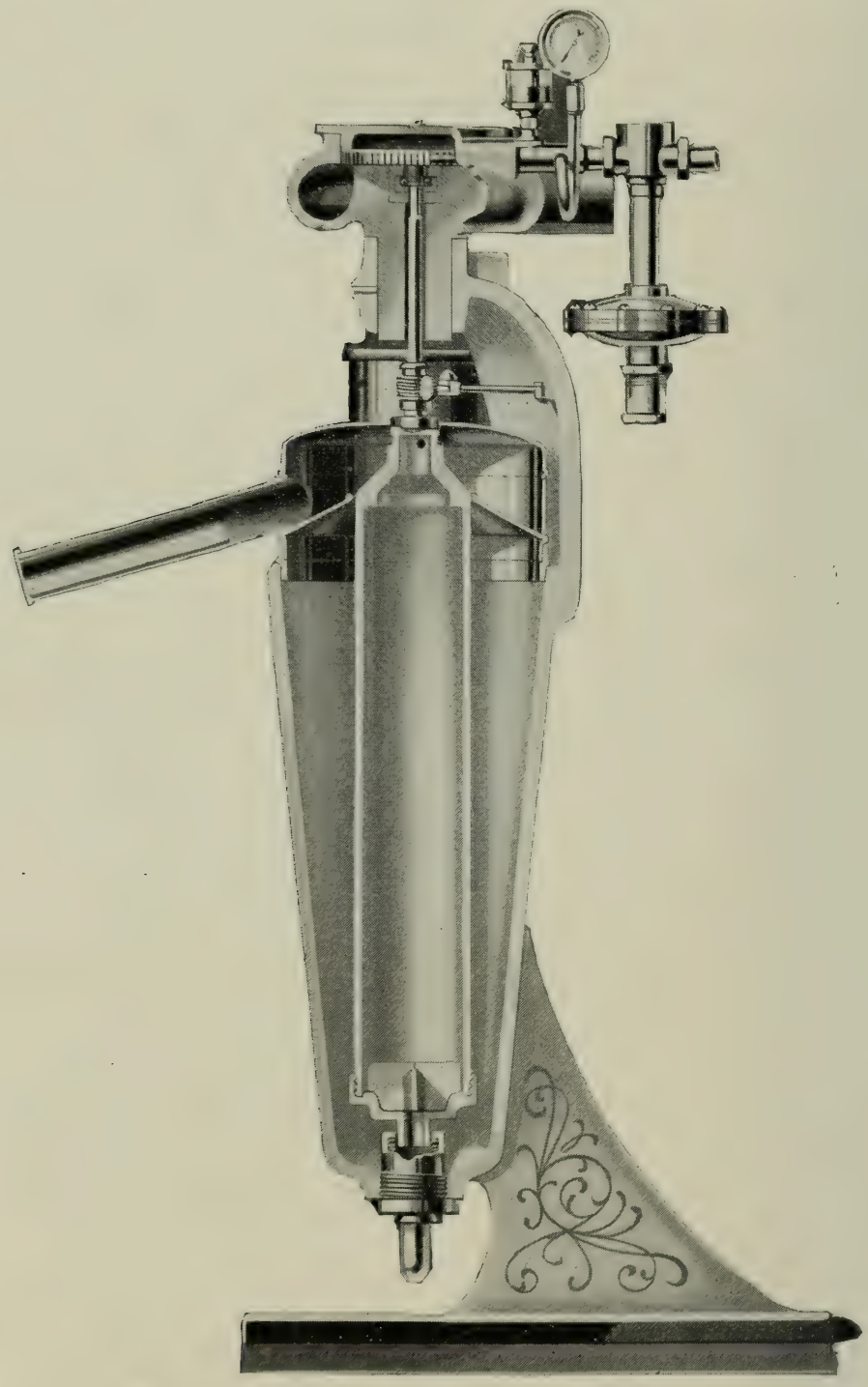


FIG. 9.—Sectional Cut, Commercial Clarifier.

FIG. 10.—Commercial Clarifier for Continuous Subsidence of Small Suspended Particles.

the difference in concentration would be in the fourth decimal place. A force of one million times gravity applied for several days should give a quite appreciable subsidence, but a complete separation would require a hundred million times the acceleration of gravity.

Recapitulation.—Particles of uniform size suspended in a liquid medium will settle by gravity when the product of their mass in grams by the constant 3×10^{16} is as large as 10. When the product is less than 10, a partial subsidence will occur until an equilibrium is established.

The only considerable force opposing complete subsidence in this case is osmotic pressure. By the use of a law of distribution, which takes into account the opposing forces, it is possible to compute the concentration at any level when the system is at equilibrium.

When the conditions are such that a particle will not settle completely by gravity, it is possible to cause complete subsidence by the application of centrifugal force when the product of the mass in grams, the force in dynes, and the constant 3×10^{13} is greater than 10. Here again a partial subsidence with the establishment of an equilibrium will occur when the product is less than 10.

The time required to establish this equilibrium can be computed from a modification of Stokes' law for velocity.

The highest commercial centrifugal force, 40,000 times gravity, can completely remove particles as small as the lower limit of ultramicroscopic observation, but such a separation will require thirty-three hours when the liquid medium is water. For less viscous media the time will be shortened. Smaller particles can never be separated by this force. Particles just too small to be retained by a fine porcelain filter can be removed in a few seconds.

Gases of different densities cannot be completely separated by any force less than about one hundred million times gravity. The same applies to true solutions. But gases should be partially separated by a much lower force within a reasonable time, whereas solutions in liquid media would require several weeks.

You may ask how all this applies in practice. I would like to mention only one instance in which the principle of centrifugal separation has been applied to very great advantage. The wool scouring plants of this country have passed many millions of gallons per week of waste liquor into public streams. The liquor is unsanitary and odorous, and the cost of purification by ordinary methods is prohibitive.

A number of these mills are now passing their liquors through a high power centrifugal at a total computed cost of about $200 per day. The wool fat recovered is being sold with gross returns of $1000 per day.

There are a number of similar instances, though I believe this is one of the most striking.

DISCUSSION.

The CHAIRMAN: It is a very wonderful paper before you, gentlemen, and I hope it will be very freely discussed. There are great possibilities, probably to every one of us, in the application of this principle which he has described. I say possibilities. Just how it will work out in practice is a question which each case will determine. I hope we will have a very free discussion of the paper. Dr. Richardson, would you care to say something?

Dr. RICHARDSON: I am extremely interested in the paper because of the fact that I have been studying the presence of colloidal material in connection with Trinidad asphalt. I wish to say that while I am not prepared to discuss the matter at this moment, I shall be very glad indeed to talk with the author personally about it.

Dr. ANDREWS: I would like to ask the speaker one question in this connection: Suppose you have a suspension which contains two colloids of different dispersity, say one consisting mainly of micron particles and another amicron particles, would the rate of subsidence of each of these in a mixture of the two be the same as it would be if those classes of particles were present without the other? In other words, would the subsidence of the one affect the subsidence of the other, or would the two subsidences take place independently?

Mr. AYRES: As far as I have been able to observe from practical experimentation, the effect of one set of particles on the other set of particles would depend in some measure on the shape of the particles. Take, for instance, very fine kieselguhr. Kieselguhr, of course, will always affect the subsidence of any other particles that may be in suspension, and will carry them down.

It seems to me, Dr. Andrews, that the question would have to be answered separately for nearly every case, because of the electro-attraction of the particles for each other, and the enmeshing and adsorbing effects of some particles on others. However, I presume

that you are discounting agglomeration crystallation in your question.

Dr. ANDREWS: I am not discounting it, because in a majority of cases it is impossible to say whether that takes place or not.

Mr. AYRES: It is true that if the time of subsidence is very long, these factors cannot be ignored. There is one point I would like to add to that, if I may. If one had a dispersion containing, as you suggested, microns and amicrons, it would be easily possible to select such a force that the amicrons would never be separated no matter how long they were left in the centrifuge, and the microns would be separated in a given time.

Dr. ANDREWS: Is that statement made as a theoretical conclusion or as a matter of observation?

Mr. AYRES: My answer is based on both. As a matter of fact it corresponds to the theory very nicely. I wouldn't want to say that I have definitely proved the accuracy of the theory by experimental work, but I have had a great many indications that that is the case. One of the things I mention in the paper is along that line. I have whirled a certain emulsion whose particles were of indeterminate size, and the degree of separation was no greater at the end of a week than at the end of fifteen minutes. I have put the same emulsion in the high-power centrifuge for one minute, and the degree of separation has been ten times as great as the degree of separation in the other machine. That seems to be one rough confirmation of the statement, that you can select a force which will remove a given size of particle, whereas a lower force will simply change the state of equilibrium.

Dr. ANDREWS: I would like to ask a second question, rather intimately connected with the first. Suppose you have a very fine suspension, not colloidal but of such a fineness that it would partly go through a filter and would subside very slowly under the action of gravity. If you add to such a solution as that a concentrated solution of ether in water, the rate of subsidence is enormously increased. Any reference to the action of electrical forces leads, therefore, to the question as to whether you would ascribe enhancement of the rate of subsidence in this case to different electrical relations of the particles in the medium, or would it be due simply to change of viscosity?

Mr. AYRES: The change of viscosity would not be nearly great enough to account for it.

Dr. ANDREWS: In what way does the surface tension affect it?

Mr. AYRES: I cannot answer. But it is my opinion that surface tension is a function of certain other factors correctly taken into account in the calculations just described.

The CHAIRMAN: Any further discussion?

Mr. DECEW: I am especially interested in the possibility of centrifugal separation of solid colloid particles from gaseous media. I know of a case where immense quantities of finely divided solids are held in suspension in air. Apparently the solids will not settle from the gas except after a long period. The solids when finally settled are separately surrounded by films of air, so that the effect is much the same as that of a liquid. One barrel of the sediment so obtained is not likely to weigh over 200 pounds, whereas a barrel of the sediment that has been packed wet and then dried will weigh anywhere from 800 to 1000 pounds. Inasmuch as it has been found impracticable to filter out this matter, I have wondered just what could be accomplished by the use of high centrifugal force.

Mr. AYRES: From your description the size of the individual particles must be comparatively large to form a visible cloud. It would, therefore, be my opinion that the material should settle rapidly by gravity if convection currents could be completely avoided. I have no doubt whatever but that the air could be completely freed from this colloidal material if it were passed through a high power centrifuge. The solids would form a very dense cake in the centrifugal rotor. But from your reference to large quantities of solids I would doubt whether any separation process non-continuous with respect to solid matter, as a high centrifugal separation is bound to be, would ever be practical for such work.

Your question suggests the idea of separation of carboniferous particles from smoke by the use of high centrifugal force. As far as I know this has never been tried but there would seem to be no great obstacles in such a procedure.

Dr. ANDREWS: Not very long ago I had the pleasure of having a conversation with Professor Frank, of Berlin, who gave me a very interesting account of just the type of separation which has been referred to here by centrifugal force. The particular problem was this: Given a mixture of hydrogen and carbon dioxide, to separate the two by a physical process. The first idea that occurred was to cool down the gas so as to freeze out the carbon

dioxide. That seemed to be very simple but it proved to be technically extraordinarily difficult. No filtering medium could be found that would filter out the carbon dioxide slowly from the hydrogen gas, because the filter would immediately clog up. A cylindrical separator was therefore devised, and it effected perfectly the separation of this extraordinarily finely divided precipitated carbon dioxide from the hydrogen gas.

Mr. AYRES: That would seem to be an ideal application. The particles were not small enough to go through the ordinary filter papers, were they?

Dr. ANDREWS: Small enough to completely stuff the pores of the finest filters.

Mr. AYRES: Therefore larger than one-tenth of a micron?

Dr. ANDREWS: You can judge that better than I can.

Dr. RICHARDSON: I merely wish to call the attention of Mr. Ayres to the effect of viscosity of Trinidad asphalt solutions. If the concentration is one per cent, the amount of colloidal matter in the liquid is two and one-half per cent. As you increase in concentration, the amount of materials in the colloidal state gradually rises until at fifty per cent concentration it is ten and seven-tenths per cent, and if you go up higher you get the asphalt itself. The amount of mineral matter at present is thirty-five and one-half per cent, which is the reason Trinidad asphalt always has some amount of mineral matter in it. It always has thirty-five and one-half per cent. I prepared a paper on this subject before the Western Society of Engineers last month. Here is a copy of it, and I would like to hear from you, as to what you think of it.

The CHAIRMAN: I would like to ask Mr. Ayres this question: He referred in one of his diagrams to cottonseed oil. I am not so familiar with cottonseed oil as linseed oil, but take linseed oil, for instance, paper filtered and perfectly clear. That oil, if put in bottles and shipped to Chicago from New York and brought back to New York unopened, without any change, will appear clouded. Apparently simple agitation produces a cloud, so that it is impossible for a manufacturer of linseed oil—raw linseed oil—not chemically treated—to insure perfect clarity for shipment. It may be clear when it leaves the factory, but it will not be clear necessarily on its receipt by the purchaser. The proposition of treating it in a machine of this kind is very interesting. The only point that arises is this: Do you think with perfectly clear oil there

will be a visible separation on passing through a machine of that kind? Take the case of cottonseed oil, which you have experimented with. Do the two fractions of oil that come from that machine differ in appearance to the eye, assuming that the original oil was practically clear?

Mr. AYRES: From your question, do I understand that you refer to a Tyndall effect in the oil visibility of light ray?

The CHAIRMAN: Yes.

Mr. AYRES: We can take an apparently clear oil, giving a slight Tyndall effect, and after passing through the machine that effect will be very greatly diminished.

The CHAIRMAN: That is on one fraction; what about the other fraction?

Mr. AYRES: In this machine you only get one fraction unless we have two discharge openings—unless we force it out from both openings for experimental purposes. If any oil is passed through the rotor it will be discharged from the periphery unless we wish it discharged from the center, accumulating any solid matter in the machine.

Mr. ALEXANDER: I understand that in crude linseed oil there is a certain amount of albumen, which is, practically speaking, in solution or very finely subdivided. It is a well-known fact that solutions of that character will very often coagulate on agitation. It seems to me that may be the cause of the difficulty you speak of, and that the difficulty may be eliminated by coagulating these substances and filtering them, possibly in a machine of this kind. I have never made any investigation.

Mr. AYRES: I am prepared to make one other observation on this. I have examined samples of linseed oil containing some material not identified by me, which, when the oil was warm, was apparently in solution—at least was not visible; when gradually cooled it was precipitated as a cloud, and after precipitation it was extremely difficult to get back in the solution.

When asked your question I wondered if this may not have happened in transit—it may have been cooled, and then simply not gone back in the solution.

The CHAIRMAN: I think that was not so in this case. We made the test in the summer time.

A DELEGATE: The result is somewhat analogous to the clouding of beer.

CORROSION OF INGOT IRON CONTAINING COBALT, NICKEL, OR COPPER

By HERBERT T. KALMUS and K. B. BLAKE

THERE have been of late, in our technical journals, voluminous discussions as to the cause and prevention of the corrosion of iron and steel.

Dr. Walker[1] says: "Corrosion is an electrolytic phenomenon and can be understood by electrical engineers on purely electrochemical grounds. It takes place at ordinary temperatures only in the presence of water through the reaction

$$Fe + 2H^{\cdot} \rightarrow Fe^{\cdot\cdot} + 2H.$$

This means that a metallic iron atom electrically neutral interacts with 2 hydrogen ions present in the water and which carry electrical charges; the result is the production of an iron ion which takes up the two electrical charges from the hydrogen ions and the deposition of the two atoms of hydrogen. Energy is lost to the surroundings and appears as electricity and heat."

In general the electrolytic theory of corrosion is accepted, according to which the principal factors which influence corrosion are: (1) the number of hydrogen ions; (2) the intimacy of contact of the hydrogen ion with the iron; (3) the solution pressure of the iron; (4) the depolarizing action of oxygen; (5) the osmotic pressure of the iron ions. Nevertheless, with these general statements in mind, there is a certain amount of dissatisfaction with the application of the electrolytic theory, particularly as it bears on prediction and remedy. Taking the electrolytic theory without

* Author's abstract of report to the Canadian Department of Mines, published by permission of the Director of Mines, Ottawa, Canada. This publication is one of a series on the metal cobalt, and its alloys, particularly with reference to finding increased usage for them, which were conducted at Queen's University, Kingston, Ontario, for the Mines Branch, Canada Department of Mines. See also Journal of Industrial and Engineering Chemistry, 6 (1914), 107 and 115; 7 (1915), 6; 7 (1915) 379.

[1] Transactions of the American Electrochemical Society, 1916, Vol. 29, p. 435.

further consideration, one would conclude that homogeneity in the material insures protection from corrosion while heterogeneity enhances corrosion. While this is doubtless true as one term of the summation of causes which bring about corrosion, nevertheless, the other terms are of sufficient importance as to frequently make this conclusion contrary to observation.

Accepting the electrolytic theory, it follows in the case of iron, other conditions being alike, that the approach to absolute freedom from impurities should add to its resistance to corrosion. It does not follow, however, that of the metals technically produced, those analyzing to have the least amount of impurities are the most non-corrosive, for other conditions are by no means always alike. Stresses or strains even in a pure metal, produced by uneven cooling of a casting or by rolling, whether hot or cold, provide unequal solution tension at various points in the metal, particularly on the surface, and would thus promote corrosion. Also in the preparation of metals of high chemical purity, the resistance to corrosion may be decreased by gas occlusion, or in other ways, in part due to the very attempt to attain the high degree of purity.

Stresses and strains are usually partially or entirely overcome by thoroughly annealing metals to be used for sheet roofing materials, or for other purposes where resistance to corrosion is of great importance. The effect of the occlusion of hydrogen in steel has been shown by a number of investigators to be very important, and under certain conditions, the volume of this gas occluded will reach nearly 50% of that of the metal itself.

It is common knowledge that two metals when alloyed often have greater resistance to corrosion than either component metal alone. The principle applies to any number of components. This is probably always due to the formation of some compound or compounds of the two metals, or under certain special conditions, it might be due to the combination of one alloying metal with the impurities of the other metal, in such a manner as to cause the solution tension of the resulting compounds to be about alike. The effect of alloying a second metal with iron or steel may also affect the corrosion of the original iron or steel by increasing or decreasing the amount of occluded hydrogen.

Another important effect of the introduction of the second metal may be to form an oxide when corrosion commences which is of such an adherent nature as to form a firm coating, inhibiting

further corrosion or preventing an excess of oxygen. Or conversely, initial rusting may render the underlying iron anodic and may accelerate corrosion, perhaps in a large measure due to the moisture held by the rust. Thus, a great deal would depend upon whether or not the rust was soft and spongy or hard and adherent.

We are inclined to agree with those who consider the phenomena of corrosion more complex than has been generally stated by enthusiastic adherents of the electrolytic theory. While this theory would seem sound as a guiding principle, much additional data will be needed before it can be amplified in detail to be accepted by engineers and practical men in general, as useful to them in prevention and prediction.

PURPOSE OF INVESTIGATION

This investigation is primarily apart from theoretical considerations and explanations, having for its fundamental purpose measurement and the setting forth of data. Several long series of measurements extending over a period of years were made on the corrosion of American Ingot Iron as affected by the additions of small quantities of cobalt, nickel, copper and carbon. We have particularly in mind the addition of small quantities of these elements to the very pure iron prepared by the open-hearth method for sheet-roofing material. Our interest was stimulated by the positive nature of certain very early preliminary experiments described in the next paragraph.

PRELIMINARY EXPERIMENTS

Very early in the course of these investigations on cobalt and its alloys, in the autumn of 1912, a preliminary set of alloys was prepared by adding small percentages of both cobalt and nickel to very pure iron. These alloys were exposed for several months on the roof of Nicol Hall, Queen's University, Kingston, Ontario. After this exposure they were removed, and the amount of corrosion determined. In every case it was found that the addition of small percentages of cobalt and nickel had decreased the corrosion of the pure iron.[2]

Following this a second set of alloys was made with the same materials, in the same way, and exposed under the same conditions

[2] The "pure" iron was American Ingot Iron, see p. 5.

as the previous set, for a period from June 16th, 1913, to October 16th, 1913. At the end of this exposure of 122 days, the alloys were taken in, and the rate of corrosion in grams per square centimeter of exposed surface per year was computed.

Unfortunately, two of the alloys of this set met with accident during the exposure, owing to dropping from the supports and coming in contact with the metal roof, so that the series is not sufficiently complete to warrant giving all the details. However, the results were in general accord with those of the previous set, which led us to believe that the addition of cobalt in proper proportions to pure iron might prove of benefit to its non-corrosive properties.

The general method of procedure with these preliminary experiments was the same as that described in detail for the complete sets of experiments to be described below.

The two sets of experiments above described must be considered preliminary for a number of reasons, primarily because no heat treatment was given to the alloys.

Conclusions

I. From these preliminary experiments it appears that additions of small percentages of both Co and Ni to American Ingot Iron add to its non-corrosive properties.

II. Cobalt seemed to be more effective than nickel when used in like amount.

III. These results were such as to stimulate further interest, but were not sufficiently complete or satisfactory to warrant drawing definite conclusions, particularly as to the relative effect of nickel and cobalt.

EXPERIMENTS ON NON-CORROSIVE ALLOYS PREPARED BY ADDITIONS OF COBALT, NICKEL, AND COPPER TO AMERICAN INGOT IRON

Following are the data of three extended series of observations, from which our conclusions are drawn. The materials used for the preparation of the alloys of all three series, and likewise the general procedure, were the same for all, and are given before the numerical corrosion values for the series.

Materials for Preparation of Alloys

The base of all the alloys was American Ingot Iron, furnished

by Dr. Beck, American Rolling Mill Company, Middletown, Ohio. The analysis of this material as furnished by Dr. Beck is:

Fe	99.9 %
S	0.023
P	0.004
C	0.010
Mn	0.031
Cu	0.028
O	0.035
Si	trace
Ni	none
Ca	none

Our own analysis of the material is as follows:

Fe	99.9 %
S	0.027
P	0.0075
C	0.010
Mn	0.027
Cu	0.048
Si	trace
Ca	none
Ni	none

Inasmuch as there has been some discussion in recent literature as to the effect of copper in adding to the non-corrosive properties of American Ingot Iron and similar materials, we have made, in addition to the usual check analyses, additional analyses of the copper content. Checking our value of copper as 0.048, we have the following values from independent analyses:

0.046
0.045
0.050

These were made by two independent analysts.

A later analysis of American Ingot Iron rolled into sheets for

roofing material and shipped to this laboratory by the American Rolling Mills, Middletown, Ohio, is as follows:

Sample No. 34175[3] American Ingot Iron Corrosion Sheet 8′×4′

S....0.026%
P....0.009
C....0.010
Mn...0.022
Cu...0.016

The cobalt, nickel and copper were of a correspondingly high degree of purity, analyzing respectively 99.7% cobalt, 99.3% nickel and 99.8% copper. The cobalt was prepared in this laboratory by reduction of purified oxide,[4] and the copper and nickel were procured from reliable sources and analyzed in this laboratory as above stated.

METHOD OF PREPARATION OF ALLOYS

Crucible and Furnace

The alloys were all made in lined graphite crucibles obtained either from the Dixon Crucible Company, or the Jonathan Bartley Crucible Company, and were either No. 3 or No. 5 size. These crucibles were lined with first grade powdered magnesite, the magnesite being mixed with water to bind it until set.

A Hoskins electric furnace of the carbon plate resistor type was used for melting these alloys.

Melting and Casting

The components of the alloy to be prepared were weighed out and put into the crucible together. Often these went into a cold furnace to be melted without pre-heating, while some were pre-heated in the Monarch Oil furnace previous to putting them into the hot electric furnace.[5] The temperature of the inside of the

[3] Analysis furnished by Dr. Beck, American Rolling Mill Co., Middletown, Ohio.

[4] "Preparation of Metallic Cobalt by Reduction of the Oxide," Herbert T. Kalmus, Bulletin No. 209, Canada Dept. of Mines, 1913. Journal of Industrial and Engineering Chemistry, VI, 107, 1914.

[5] For a description of this furnace see "Preparation of Metallic Cobalt by Reduction of the Oxide." Herbert T. Kalmus, Bulletin No. 259, Canada Dept. of Mines, 1913.

furnace was measured from time to time, observations being made with a Wanner optical pyrometer.

After the melt had received what was considered a proper furnace treatment, powdered aluminium was added as a degasifier and it was then poured into an iron mold of variable depth which formed a cylindrical ingot about 1.25″ in diameter. The alloys of Series I and II were cast in a square mold of about the same volume. The casting usually weighed in the neighborhood of 2 lbs.

The alloy was not considered satisfactory unless the crucible lining remained intact throughout, and after the furnace treatment showed no evidence of having allowed the melt to come in contact with the crucible itself.

Preparation of Discs for Corrosion Tests

The castings obtained were usually from 4″ to 6″ long, and about 1.25″ in diameter. To obtain samples for corrosion tests, these bars were turned into discs and when satisfactory the discs were annealed in a Fletcher Russell Muffle furnace.[6] For the small samples this was done by heating them at 780° C. for two hours and allowing them to cool with the furnace during a period of six hours.

After turning off a skin to get rid of the outside layer, the disc was finished to a smooth surface in a lathe, the finishing chips being kept for analysis for carbon. After this operation the discs were carefully polished on the buffing lathe and measured for exposure.

Method of Measurement to Determine Corrosion

Before exposure several careful measurements were made of the diameter and thickness of the discs, the average of the measurements being taken to compute the surface. In the first preliminary set of experiments the discs were then carefully weighed, washed in alcohol to remove any form of grease, and suspended by silk threads from pegs set in a large board. The board with samples was placed on the roof. In the second preliminary set of experiments and in Series I to III, instead of suspending the discs by silk cords, holes were drilled in one edge of them and vulcanized fiber pegs were tightly inserted. These were mounted on frames as shown in illustrations, Plates I and I*a*. This method of support was resorted to as the silk cords proved unreliable, and on several

[6] Series I and II were not annealed; Series III, IV and V were annealed.

occasions, after particularly severe weather conditions, samples were ruined by falling to the metal roof.

After corroding a sufficient length of time, the samples were brought in and the rust removed with a 20% solution of ammonium citrate. They were then carefully washed in alcohol, dried and weighed, and the loss in weight determined as shown in the tables to follow.

Absorption of Carbon Monoxide Gas

Although no carbon or carbon-containing materials were added to the charge, it was always found upon analyzing the alloy that the carbon content had increased, in one case from 0.010% to 0.48%. The conclusion seems to be that carbon was absorbed from the CO atmosphere within the furnace. The action did not seem to be uniform under the conditions that prevailed, and we did not determine the conditions which governed the rate of absorption of carbon by the metal. It was definitely observed that it was far more difficult to obtain low carbon castings of the cobalt-iron alloys than it was to obtain low carbon castings of either the nickel-iron or copper-iron alloys.

CORROSION TESTS

Series I

The following alloys comprising Series I were exposed on the roof of Nicol Hall, as described above, beginning at 10 o'clock on the morning of March 18th, 1914. They were removed on August 31st, 1914, after an exposure of 3984 hours.

Alloy H 202 — December 2nd, 1913.

Iron..... 99.75% Cobalt..... 0.25%

Furnace—Hoskins Resistor Furnace, type F.C. No. 105.
Crucible—Graphite No. 3, magnesite lined.
Soaking—Duration 30 mins.
Temperature 1500° C.
Maximum Temperature reached by melt, 1560° C.
Degasifier—Powdered aluminium 0.2 gram.
Heat Treatment—None.

The crucible, furnace, heat treatment, soaking, and degasifier for the following set of alloys were identical with that given under alloy H 202:

Alloy H 209 — February 10th, 1914.
Iron..... 99.5% Cobalt..... 0.5%

Alloy H 198 — November 25th, 1913.
Iron..... 99.0% Cobalt..... 1.0%

Alloy H 199 — November 25th, 1913.
Iron..... 98.0% Cobalt..... 2.0%

Alloy H 200 — November 26th, 1913.
Iron..... 97.0% Cobalt..... 3.0%

Alloy H 204 — November 29th, 1913.
Iron..... 99.75% Nickel..... 0.25%

Alloy H 207 — February 4th, 1914.
Iron..... 99.5% Nickel..... 0.5%

Alloy H 195 — November 22nd, 1913.
Iron..... 99.0% Nickel..... 1.0%

Alloy H 196 — November 23rd, 1913.
Iron..... 98.0% Nickel..... 2.0%

Alloy H 197 — November 24th, 1913.
Iron..... 97.0% Nickel..... 3.0%

Alloy H 205 — November 28th, 1913.
Iron..... 99.75% Copper..... 0.25%

Alloy H 206 — February 4th, 1914.
Iron..... 99.5% Copper..... 0.5%

Alloy H 208 — December 2nd, 1913.
Iron..... 99.0% Copper..... 1.0%

Alloy H 216
Iron..... 100.0%

There are also included in this set several alloys made up under mill conditions by the American Rolling Mill Company, having compositions shown below. These alloys were given only the standard heating necessary in the rolling process and no special heat treatment:

Alloy 34204

Co	0.35 %	P	0.009%
C	0.010	S	0.022
Mn	0.020	Cu	0.020

Alloy 34196

Co	0.60 %	P	0.008
C	0.010	S	0.040
Mn	0.020	Cu	0.024

Alloy 34185

Co	1.18 %	P	0.006%
C	0.015	S	0.034
Mn	0.017	Cu	0.028

Alloy 44009

Ni	0.75 %	P	0.008%
C	0.010	S	0.025
Mn	0.015	Cu	0.24

Alloy 34175

C	0.010%	S	0.026%
Mn	0.022	Cu	0.016
P	0.009		

These alloys, including the one sample of iron No. 34175, came into our hands in the form of partially rolled sheets, approximately ½" in thickness and 10" in width. The discs were cut directly from these sheets and after the proper measurements were exposed in the manner already described for Series I.

The following table shows the corrosion of these various alloys:

SERIES I

LOSS IN WEIGHT OF DISC

(Exposure 3984 hours)

Sample Number of Alloy.	Approximate Analysis. Per Cent.	Loss of Weight in Grams Due to Corrosion.	Corrosion or Loss in Weight in Grams per Sq. Cm. of Original Surface per Hr. $\times 10^8$.	Remarks.
H 204 (*a*)	Fe 99.75 Ni 0.25	0.5879	752	Rust quite tenacious.
H 204 (*b*)	.....	0.4994	696	
H 207 (*a*)	Fe 99.5 Ni 0.50	0.3844	510	Rust quite tenacious.
H 207 (*b*)	Same	0.4413	717	
H 207 (*c*)	Same	0.4740	905	
H 195 (*a*)	Fe 99.0 Ni 1.0	0.5478	694	Rust tenacious. Sample (*b*) had fallen off.
H 195 (*b*)	.....	0.7069	928	
H 196 (*a*)	Fe 98.0 Ni 2.0	0.5304	681	Rust removed somewhat easier than H 195.
H 196 (*b*)	.....	0.5466	687	
H 197 (*a*)	Fe 97.0 Ni 3.0	0.4642	589	Rust easily removed.
H 197 (*b*)	.....	0.4674	652	
H 202 (*a*)	Fe 99.75 Co 0.25	0.4533	673	Rust removed easier than H 200. Sample (*b*) had fallen off.
H 202 (*b*)	.....	0.5543	775	
H 209 (*a*)	Fe 99.5 Co 0.5	0.5518	793	Both samples off. Rust removed quite easily. Sample (*b*) had not rusted much on one side.
H 209 (*b*)		0.3658	478	
H 198 (*a*)	Fe 99.0 Co 1.0	0.5822	722	Rust dark in color and more tenacious than H 197.
H 198 (*b*)	.....	0.5661	720	
H 199 (*a*)	Fe 98.0 Co 2.0	0.5323	774	Rust dark in color and tenacious.
H 199 (*b*)	.....	0.5574	768	Rust dark in color and quite tenacious.

SERIES I—*Continued*

LOSS IN WEIGHT OF DISC

Sample Number of Alloy.	Approximate Analysis. Per Cent.	Loss of Weight in Grams Due to Corrosion.	Corrosion or Loss in Weight in Grams per Sq. Cm. of Original Surface per Hr. $\times 10^8$.	Remarks.
H 200 (*a*)	Fe 97.0 Co 3.0	0.3836	482	Rust dark in color and quite tenacious.
H 200 (*b*)	.....	0.4882	627	
H 205 (*a*)	Fe 99.75 Cu 0.25	0.5245	738	Rust about the same as H 204.
H 205 (*b*)	.....	0.5379	734	
H 206 (*a*)	Fe 99.5 Cu 0.50	0.5677	761	Rust about the same as H 204.
H 206 (*b*)	.....	0.5601	748	
H 208 (*a*)	Fe 99.0 Cu 1.0	0.4630	816	Rust removed rather easily.
H 208 (*b*)	.....	0.4565	835	
H 216 (*a*)	Fe 100.0	1.0332	968	Rust quite easily removed. Sample (*a*) had fallen off.
H 216 (*b*)	.....	1.0966	937	
34175 (*a*)	S 0.027 Mn 0.027 P 0.0078 Cu 0.020 C 0.131	0.9331	664	Rust removed very easily.
34175 (*b*)	.....	0.9582	701	Rust removed very easily.
34204 (*a*)	Co 0.22 S 0.022 Mn 0.036 P 0.0058 Cu 0.020 C 0.125	1.6014	1180	Rust more tenacious than 34,175, but less tenacious than 34,185.
34204 (*b*)	.....	0.3968	292	
34196 (*a*)	Co 0.57 S 0.025 Mn 0.031 P 0.0097 Cu 0.021 C 0.156	0.8752	640	Rust tenacious; not very different from 34,204.
34196 (*b*)	.....	0.9518	678	

SERIES I—*Continued*

LOSS IN WEIGHT OF DISC

Sample Number of Alloy.	Approximate Analysis. Per Cent.	Loss of Weight in Grams Due to Corrosion.	Corrosion or Loss in Weight in Grams per Sq. Cm. of Original Surface per Hr. $\times 10^3$.	Remarks.
34185 (*a*)	Co 1.09 S 0.027 Mn 0.032 P 0.008 Cu 0.025 C 0.145	1.1118	823	Rust very tenacious.
34185 (*b*)	.....	0.8129	613	Rust very tenacious.
44009 (*a*)	Ni 0.70 S 0.020 Mn 0.025 P 0.006 Cu 0.27 C 0.140	1.0528	783	Rust very tenacious; not unlike 34,185.
44009 (*b*)	.....	1.1238	819	

Microphotographs. No microphotographs were taken of the alloys of the above Series I.

CONCLUSIONS

Corrosion Experiments, Series I

I. Results with the set of alloys, numbers 196 to 216, show that in every case the alloy formed by the addition of cobalt, nickel, or copper is less corroded in the atmosphere than is American Ingot Iron. These are unannealed samples.

II. The variations in the measurements are not so great but that conclusion is apparent, but they are sufficiently great that conclusions based upon a comparison of the alloys among themselves could not be drawn from this series alone.

III. The samples, numbers 34175 to 44009, prepared by the American Rolling Mill Company, show such wide variations among the observations that no conclusions could be drawn from this series alone.

IV. It is noticeable throughout this series that the rust on the cobalt samples is more tenacious than that of the other samples, and particularly that it is of a much darker color and very much

more difficultly removed by mechanical means than that formed on the American Ingot Iron.

Series II

The alloys H 195 to H 216 used for Series II were identical with those used for Series I. That is to say, after completing the measurements for Series I the discs were again polished, reweighed, and measured for a second exposure.

In this second series the American Rolling Mill alloys of the

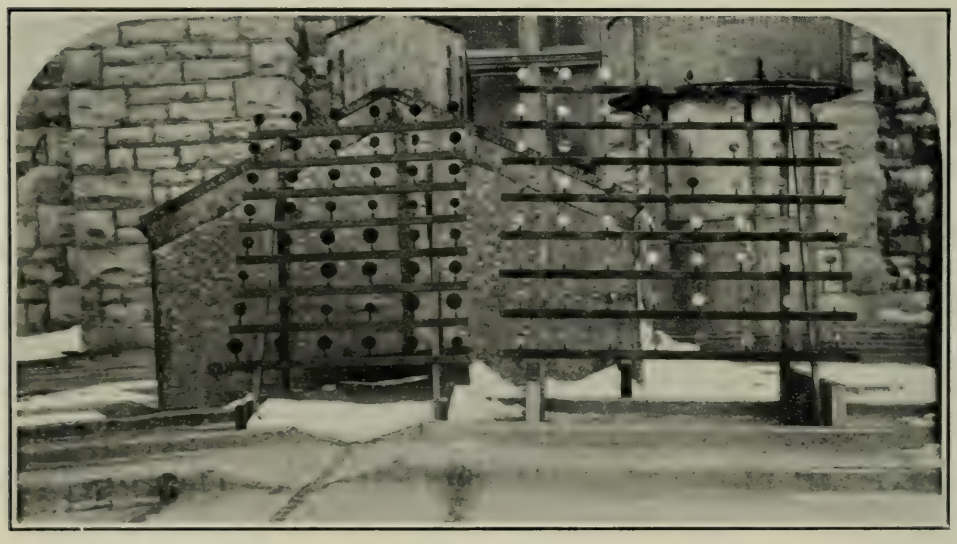


Fig. 1.

first series were again exposed, but in this case the samples were not the identical ones used in the previous test, new samples being cut from the sheets and prepared for exposure in the manner described.

The frame of samples were placed on the Nicol building of Queen's University, Kingston, Ontario, on the morning of October 10th, 1914, and removed on August 30, 1915.

The results of this second exposure may be noted in the following table:

Series II

LOSS IN WEIGHT OF DISC

(Exposure, 7776 hours)

Sample Number of Alloy.	Approximate Analysis. Per Cent.	Loss of Weight in Grams Due to Corrosion.	Corrosion or Loss in Weight in Grams per Sq. Cm. of Original Surface per Hr. $\times 10^3$.	Remarks.
H 204 (*a*)	Fe 99.75 Ni 0.25	0.6712	491	Rust difficult to remove; dark in spots and mottled in appearance.
H 204 (*b*)	.....	0.6100	470	
H 207 (*a*)	Fe 99.5 Ni 0.5	0.6033	420	Rust fairly tenacious.
H 207 (*b*)	.....	0.4979	470	
H 207 (*c*)	.....	0.3987	402	
H 195 (*a*)	Fe 99.0 Ni 1.0	0.7268	512	Rust tenacious; somewhat dark after removal of rust.
H 195 (*b*)	.....	0.6410	445	
H 196 (*a*)	Fe 98.0 Ni 2.0	0.6488	437	Rust easily removed; light in color.
H 196 (*b*)	.....	0.6606	437	
H 197 (*a*)	Fe 97.0 Ni 3.0	0.5983	401	Rust easily removed; light in color.
H 197 (*b*)	.....	0.5494	402	
H 202 (*a*)	Fe 99.75 Co 0.25	0.4939	386	Rust dark, tenacious, and uniform over surface.
H 209 (*a*)	Fe 99.5 Co 0.5	0.6495	435	Rust medium dark in color.
H 209 (*b*)	.....	0.6253	500	
H 198 (*a*)	Fe 99.0 Co 1.0	0.7590	498	Rust dark in color; fairly difficult to remove.
H 198 (*b*)	.....	0.7018	475	
H 199 (*a*)	Fe 98.0 Co 2.0	0.6364	489	Medium dark in color.
H 199 (*b*)	.....	0.6771	482	
H 200 (*a*)	Fe 97.0 Co 3.0	0.5392	356	Rust easily removed; metal underneath dark in color.
H 200 (*b*)	.....	0.5497	371	

SERIES II—*Continued*

LOSS IN WEIGHT OF DISC

Sample Number of Alloy.	Approximate Analysis. Per Cent.	Loss of Weight in Grams Due to Corrosion.	Corrosion or Loss in Weight in Grams per Sq. Cm. of Original Surface per Hr. $\times 10^8$.	Remarks.
H 205 (*a*)	Fe 99.75 Cu 0.25	0.5718	414	Rust hard in spots, metal underneath dark in color.
H 205 (*b*)	.....	0.5932	415	
H 206 (*a*)	Fe 99.5 Cu 0.5	0.6208	443	Rust light in color; metal underneath pitted.
H 206 (*b*)	.....	0.6222	442	
H 208 (*a*)	Fe 99.0 Cu 1.0	0.4112	387	Rust very uniformly distributed and difficult to remove.
H 208 (*b*)	.....	0.4034	392	
H 216 (*a*)	Fe 100.0	1.2256	608	Rust easily removed.
H 216 (*b*)	.....	1.5545	698	
34175 (*a*)	S 0.027 Mn 0.027 P 0.0078 Cu 0.020 C 0.131	0.9122	583	Rust light in color and fairly easily removed.
34175 (*b*)	S 0.027 Mn 0.027 P 0.0078 Cu 0.020 C 0.131	0.7562	592	Rust light in color and fairly easily removed.
34175 (*c*)	.....	0.6293	591	
34175 (*d*)	.....	0.5400	605	
34204 (*a*)	Co 0.22 S 0.022 Mn 0.036 P 0.058 Cu 0.020 C 0.125	1.4989	567	Rust light in color and fairly easily removed.
34204 (*b*)	.....	1.4000	594	
34204 (*c*)	.....	1.1942	604	
34204 (*d*)	.....	1.0831	562	

SERIES II—*Continued*

LOSS IN WEIGHT OF DISC

Sample Number of Alloy.	Approximate Analysis. Per Cent.	Loss of Weight in Grams Due to Corrosion.	Corrosion or Loss in Weight in Grams per Sq. Cm. of Original Surface per Hr. $\times 10^8$.	Remarks.
34196 (*a*)	Co 0.57 0.025 Mn 0.031 P 0.0097 Cu 0.021 C 0.156	0.9461	593	Rust light in color and fairly easily removed.
34196 (*b*)	.....	0.8391	613	
34196 (*c*)	.....	0.7173	609	
34196 (*d*)	.....	0.5764	592	
34185 (*a*)	Co 1.09 S 0.027 Mn 0.032 P 0.008 Cu 0.025 C 0.145	1.3278	534	Rust light in color and fairly easily removed.
34185 (*b*)	.....	1.2176	551	Rust light in color and fairly easily removed.
34185 (*c*)	.....	1.0583	539	
34185 (*d*)	.....	0.9805	570	
44009 (*a*)	Ni 0.700 S 0.020 Mn 0.025 P 0.006 Cu 0.270 C 0.140	1.0574	779	Rust light in color and fairly easily removed.
44009 (*b*)	.....	0.9182	788	
44009 (*c*)	.....	0.8464	860	
44009 (*d*)	.....	0.7179	883	

Microphotograph. No microphotographs were taken of Series II.

CONCLUSIONS

Corrosion Experiments, Series II

I. Results with the set of alloys, numbers 196 to 216, show that in every case the alloy formed by the addition of cobalt, nickel or copper is less corroded in the atmosphere than is American Ingot Iron. These are unannealed samples.

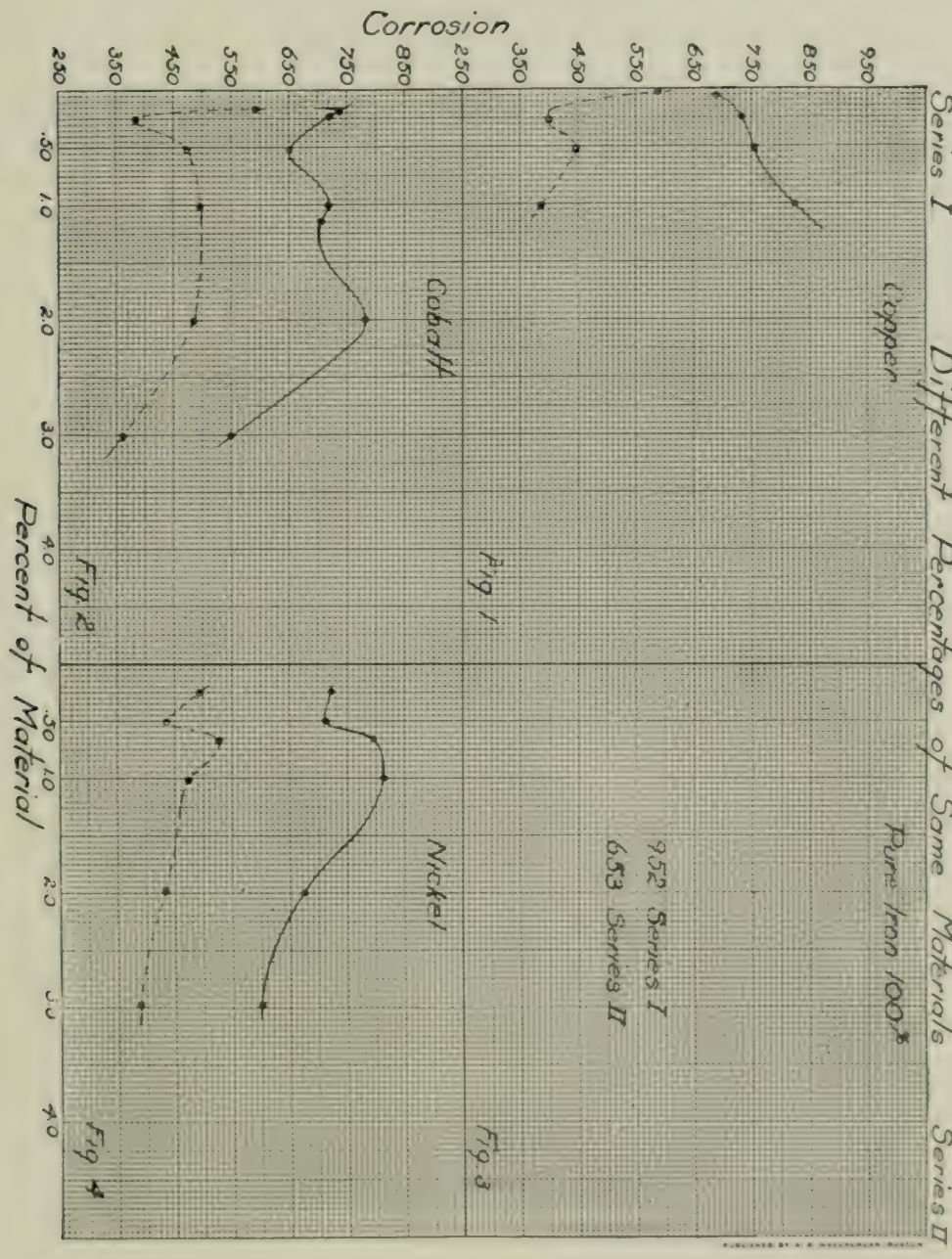
II. The conclusion I for Series II is in accord with the corresponding conclusion for Series I. Comparing the absolute amounts of corrosion; that is, loss in weight in grams per square centimeter of original surface per hour, we find that it is uniformly greater for Series I than for Series II approximately in the ratio of three to two. This may be in some measure accounted for by the fact that the exposure for Series I was largely in the summer time, whereas the exposure for Series II was both summer and winter. It is more to be accounted for, however, by the fact that the exposure for Series II was of very nearly twice the duration that it was for Series I. After corrosion has continued for a certain period, especially with the more non-corrosive alloys, a hard, tenacious, dark-colored rust is formed which tends to protect the alloy against further corrosion.

III. The variations in the measurements are not so great but that conclusions I and II are apparent, but the variations are sufficiently great that comparisons between the various alloys cannot be drawn without further confirming measurements to establish the law.

IV. The samples, numbers 34175 to 44009, prepared by the American Rolling Mill Company, so far as this series alone is concerned, do not tend to bear out the conclusions from the series prepared at the laboratory. From these samples alone it would seem that additions of small amounts of cobalt, up to one per cent, have very little effect on corrosion, and that the addition of nickel to about 0.7 per cent was harmful.

V. It is noticeable throughout this series, for the samples prepared at the laboratory, that additions of cobalt, nickel and copper all tend to make the rust more tenacious, darker in color, more uniform, and very much more difficultly removed by mechanical means than is the case with the pure American Ingot Iron. This fact is particularly noticeable with the cobalt samples. The alloys of this series, prepared by the American Rolling Mill Company,

differ from those prepared by us; that is to say, the annealed samples differ from the unannealed samples, in that for the annealed



samples the rust is light in color and much more readily removed than for the unannealed ones.

VI. From this series alone we would tend to conclude that the effect of annealing these alloys was to promote corrosion.

Series III

The samples of Series I and II received no heat treatment after casting. For further investigation Series III was prepared, the following being a list of the alloys of this series with the method of preparation:

Furnace Treatment

In this series of alloys, Series III, some of the charges were preheated in the Monarch Oil Furnace and some were introduced directly into the hot Hoskins furnace. The data of preparation of two alloys by each treatment are given in detail. All samples of this series were prepared by one or the other of these methods as indicated. Any variation from these will be noted under the individual charge.

Alloy B 202 October 23, 1914.

Composition—Co, 0.25%; C, 0.18%; Fe, 99.5%.

Charge—American Ingot Iron, 1132 grams.
Cobalt, 2.85 grams.

Furnace—Hoskins Resistor furnace, type F. C. No. 105.

Crucible—No. 3 graphite, magnesite lined.

Soaking—30 minutes at 1570° C.

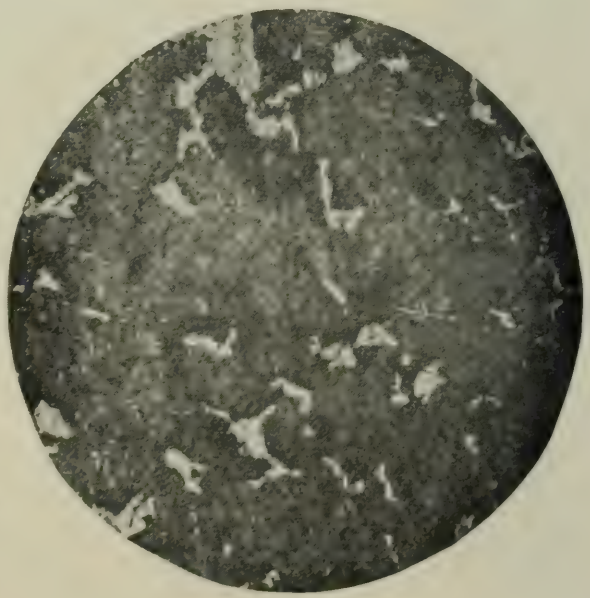
Highest Temperature reached by melt, 1570° C.

Temperature of Pouring—1570° C.

Degasifier—0.2 gr. powdered aluminium.

Weight of Casting—1 lb. 4 oz.

Heat Treatment—Annealed by heating in gas muffle furnace at 870° for 2 hours and allowing to cool slowly with furnace.



Alloy B 202.

Alloy B_2 199 November 11, 1914.

Composition—Co, 2.0%; C, 0.16%; Fe, 97.8%.

Charge—S 262, 603 grams.
Cobalt, 6.15 grams.

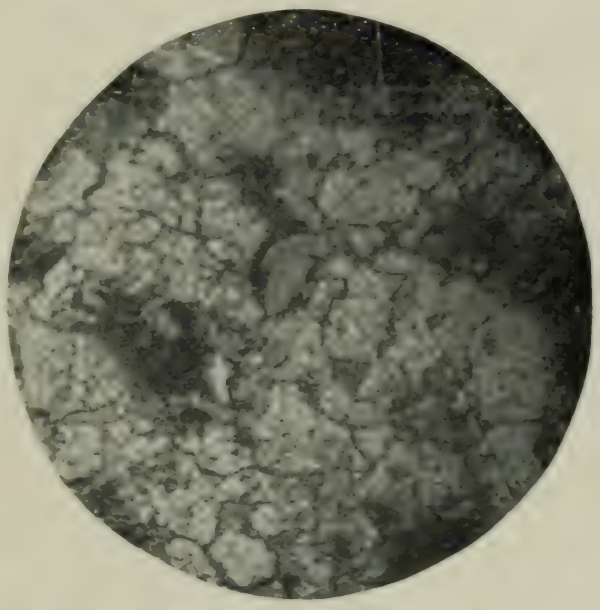
Soaking—30 minutes at 1640° C.

Highest Temperature reached by melt, 1700° C.

Temperature of Pouring—1640° C.

Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B 202, except as noted.

Weight of Casting—1 lb.



Alloy B_2 199.

Alloy S 250 November 2nd, 1914.

Composition—Co, 0.25%; C, 0.83%; Fe, 99.6%.

Charge—American Ingot Iron, 778 grams.
Cobalt, 1.95 grams.

Furnace—Hoskins Resistor furnace, type F. C. No. 105.
Monarch Oil furnace.

Crucible—No. 3 graphite magnesite lined.

Preheating—20 minutes in oil furnace.

Soaking—30 minutes at 1560° C.

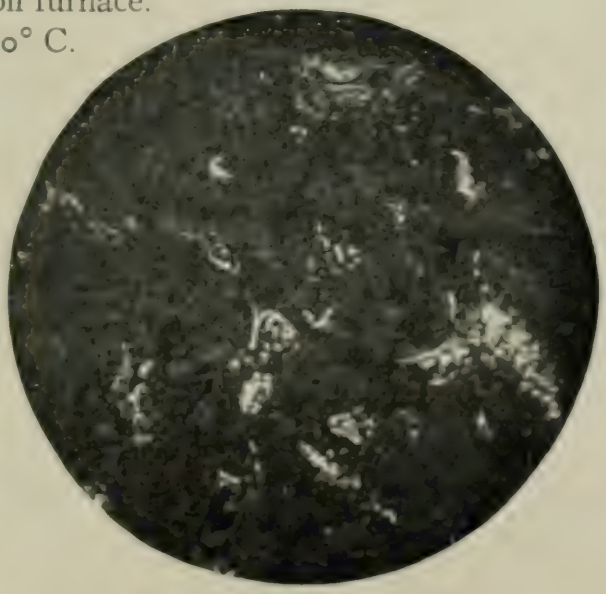
Highest Temperature reached by melt, 1640° C.

Temperature of Pouring—1560° C.

Degasifier—Powdered aluminium 0.2 grams.

Weight of Casting—1 lb. 3 oz.

Heat Treatment—Annealed by heating in gas muffle furnace at 870° C. for 2 hours and allowing to cool slowly with furnace.



Alloy S 250.

Alloy S 262 November 10th, 1914

Composition—Co, 1.0%; C, 0.62%; Fe, 98.3%.

Charge—S 260, 456 grams.
S 255, 226 grams.
B 200, 130 grams.
Pure Cobalt, 0.49 gram.

Preheating—30 minutes in oil furnace.

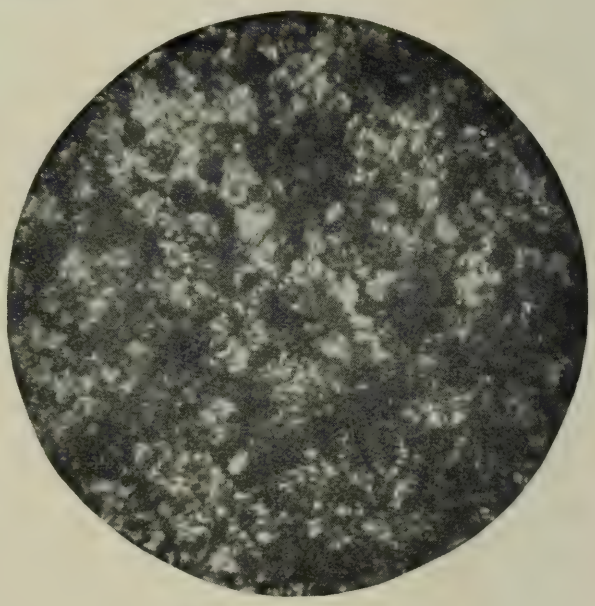
Soaking—30 minutes at 1640° C.

Highest Temperature reached by melt, 1700° C.

Temperature of Pouring—1640° C.

Crucible, Furnace Treatment, Degasifier and Heat Treatment same as S 250, except as noted.

Weight of Casting—1 lb. 5 oz.



Alloy S 262.

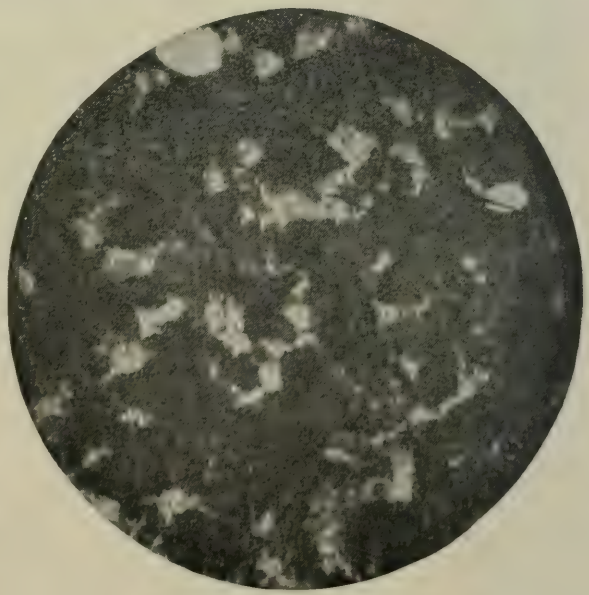
Alloy S 255 November 9th, 1914.

Composition — Co, 0.35%; C, 0.21%; Fe, 99.4%.

Charge — S 252, 375.5 grams.
American Ingot Iron, 594 grams.
Cobalt, 1.53 grams.

Crucible, Furnace Treatment, Degasifier and Heat Treatment same as S 262.

Note—This alloy was cast four times before obtaining a satisfactory sample.



Alloy S 255.

Alloy S 265 December 9th, 1914.

Composition—Co, 0.35%; C, 0.30%; Fe, 99.3%.

Charge—Alloy 32404, 467 grams.
Cobalt, 0.16 gram.

Crucible, Furnace Treatment, Degasifier, Weight of Casting and Heat Treatment same as B_2 199, with exception that duration of soaking was 1 hour.

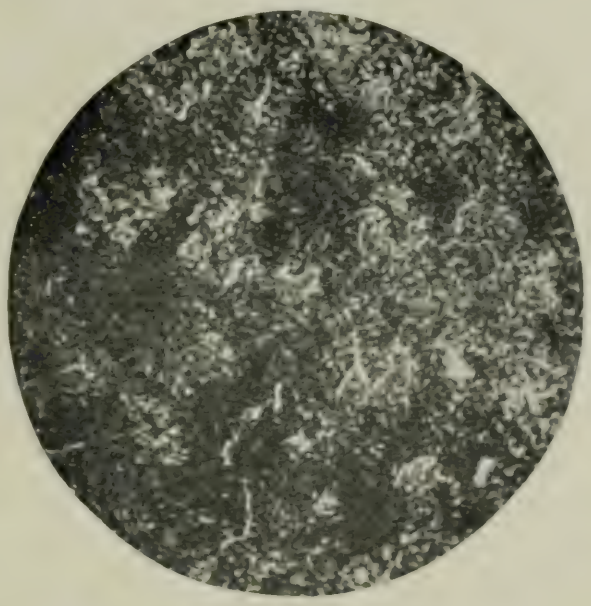
Alloy B 209 October 26th, 1914.

Composition — Co, 0.50%; C, 0.27%; Fe, 99.2%.

Charge—B 202, 637 grams.
Cobalt, 1.54 grams.

Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B 202.

Weight of Casting—1 lb.



Alloy B 209.

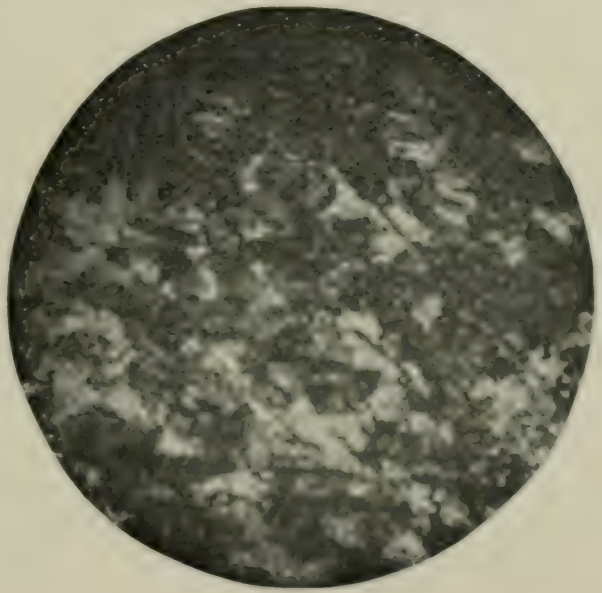
Alloy S 252 November 2nd, 1914.

Composition—Co, 0.50%; C, 0.31%; Fe, 99.1%.

Charge—S 250, 650 Grams.
Cobalt, 1.55 grams.

Crucible, Furnace Treatment, Degasifier and Heat Treatment same as S 250.

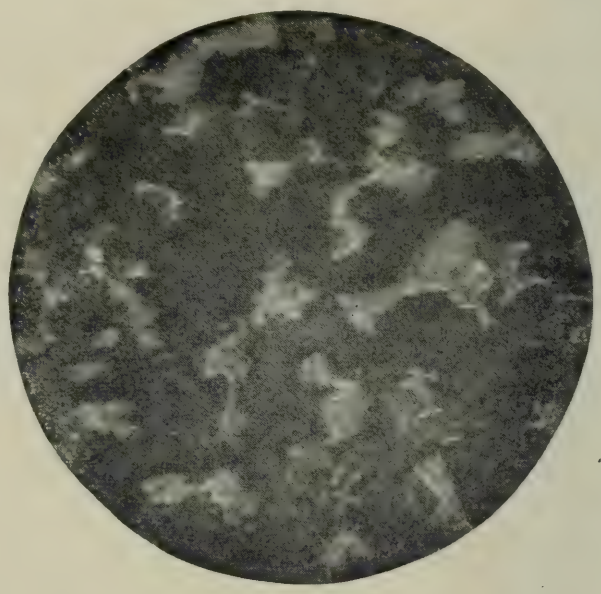
Weight of Casting—1 lb. 2 oz.



Alloy S 252.

Alloy S 260 November 9th, 1914.

Composition—Co, 0.75%; C, 0.17%; Fe, 99.1%.
Charge—American Ingot, 576 grams.
B 200, 192 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as S 262.
Weight of Casting—1 lb. 5 ozs.



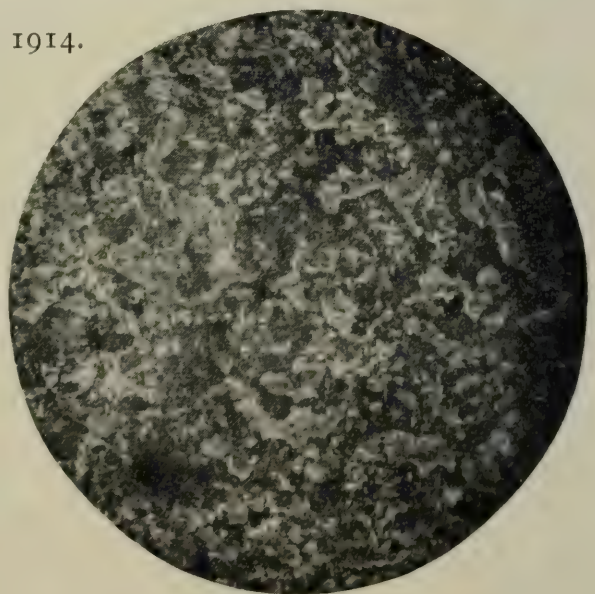
Alloy S 260.

Alloy S 266 December 9th, 1914.
Composition—Co, 0.75%; C, 0.21%; Fe, 99.0%.
Charge—Alloy 34204, 467 grams.
Cobalt, 2.35 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B_2 199, with exception that duration of soaking was 2 hours.
Weight of Casting—1 lb.

Note.—This alloy was cast twice before obtaining a satisfactory sample.

Alloy B 198 October 27th, 1914.

Composition—Co, 1.0%; C, 0.38%; Fe, 98.6%.
Charge—B 202, 326 grams.
B 209, 460 grams.
Cobalt, 4.80 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B 202.
Weight of Casting—1 lb. 8 oz.



Alloy B 198.

Alloy B_2 200 November 13th, 1914.

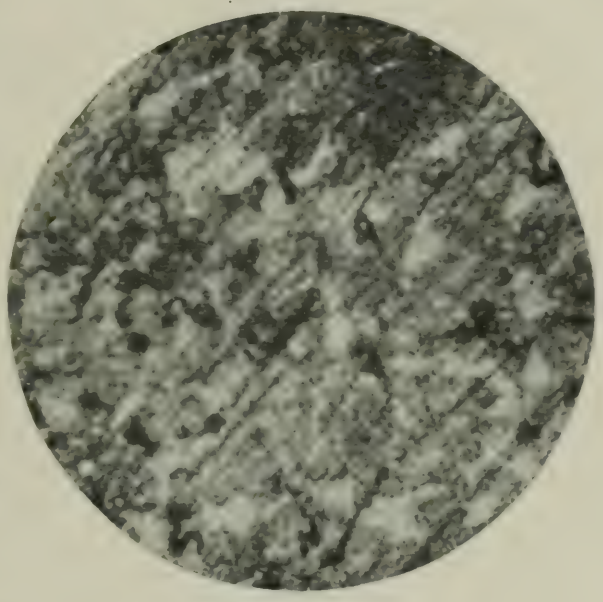
Composition — 3.0%; C, 0.17%; Fe, 96.8%.

B_2, 199, 340 grams.
American Ingot Iron 262 grams.
Cobalt, 11.63 grams.

Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B_2 199.

Weight of Casting—1 lb. 2 oz.

Note—This alloy was cast twice before obtaining a satisfactory sample.



Alloy B_2 200.

Alloy B 200 November 29th, 1914.

Composition—Co, 3.0%; C, 0.36%; Fe, 96.6%.

Charge—B 199, 482 grams.
Cobalt, 1.49 grams.

Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B 202, except highest temperature was 1680° C.

Weight of Casting—1 lb.

Alloy C 202 November 25th, 1914.

Composition—Co, 0.25%; C, 0.49%; Fe, 99.2%.

Charge—B 30, 255 grams.
American Ingot Iron, 255 grams.
Cobalt, 1.28 grams.

Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B_2, 199, with the exception that duration of soaking was 50 minutes.

Weight of Casting—1 lb.

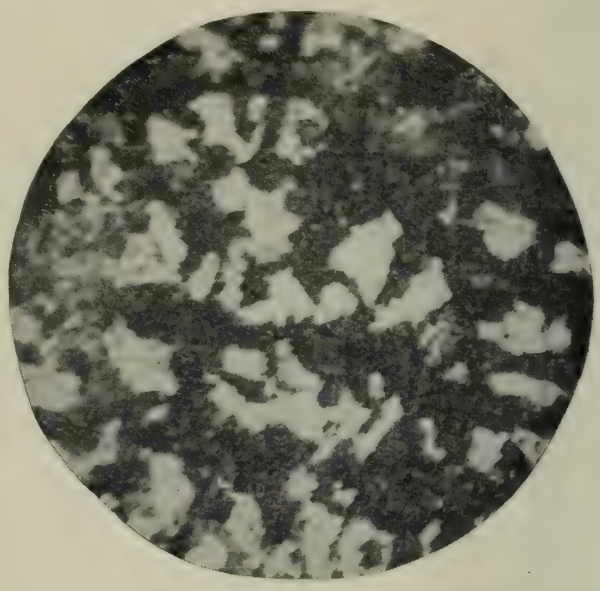
Alloy C 202.

Alloy C 255 November 26th, 1914.

Composition—Co, 0.35%; C, 0.49%; Fe, 99.2%.

Charge—C 202, 305 grams.
B 30, 85 grams.
Alloy 32404, 85 grams.
Cobalt, 0.69 gram.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B_2 199, with the exception that duration of soaking was 1 hour.

Weight of Casting—15 oz.



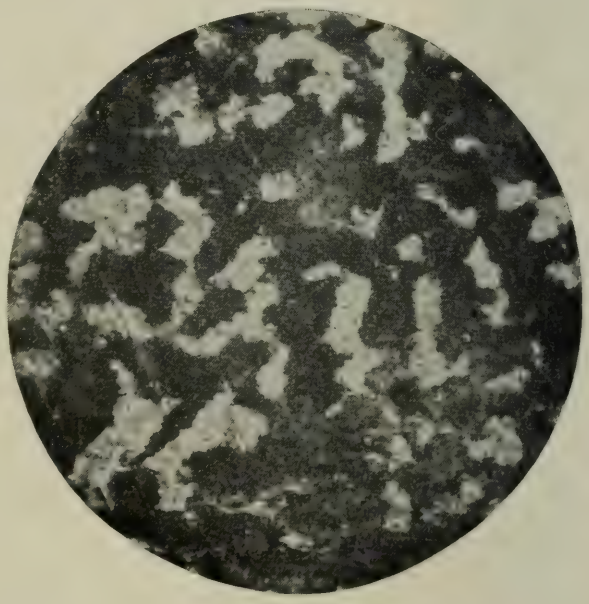
Alloy C 255.

Alloy B 199 October 28th, 1914.

Composition — Co, 2.0%; C, 0.46%; Fe, 97.5%.

Charge—B 198, 651 grams.
Cobalt, 0.65 gram.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B 202.

Weight of Casting—1 lb. 5 oz.



Alloy B 199.

Alloy B 204 October 23d, 1914.

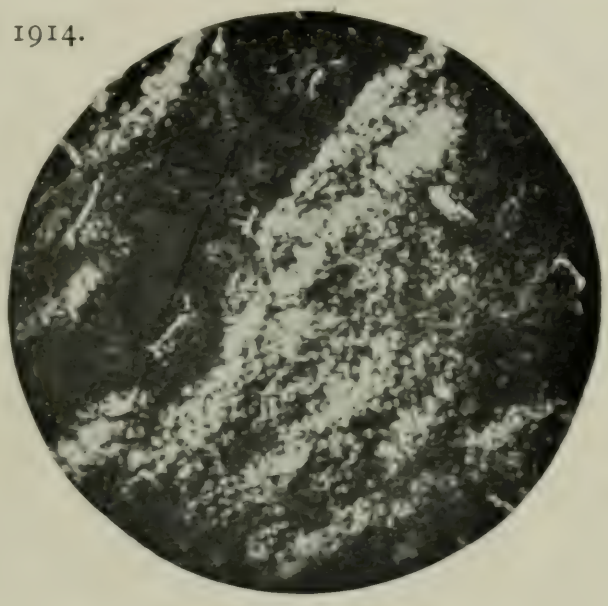
Composition—Ni, 0.25%; C, 0.10%; Fe, 99.6%.

Charge—American Ingot Iron, 1132 grams.
Pure Nickel, 2.85 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B 202.

Weight of Casting—1 lb. 4 oz.

Alloy S 251 Nov. 2nd, 1914.

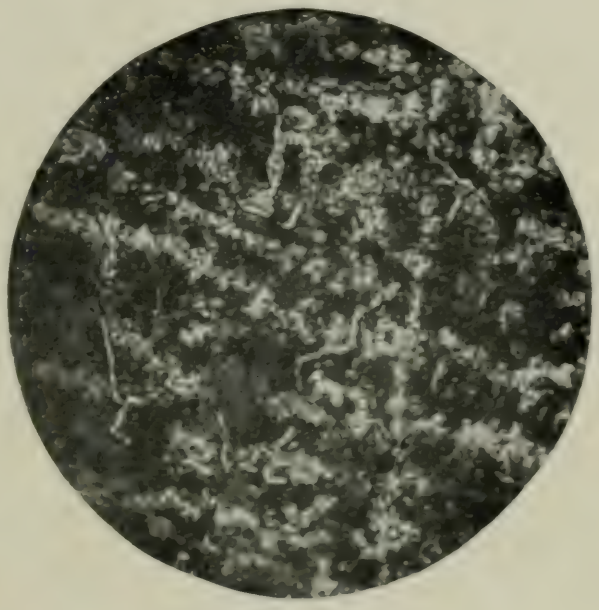
Composition—Ni, 0.25%; C, 0.057%; Fe, 99.6%.
Charge—American Ingot Iron, 743 grams.
Nickel, 1.86 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as S 250.
Weight of Casting—1 lb. 4 oz.



Alloy S 251.

Alloy S 254 November 5th, 1914.

Composition—Ni, 0.35%; C, 0.045%; Fe, 99.6%.
Charge—American Ingot Iron, 913 grams.
Nickel, 3.19 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as S 262, expect that highest temperature was 1640° C.
Weight of Casting—1 lb.
Note.—This bar was cast seven times before obtaining a perfect casting.



Alloy S 254.

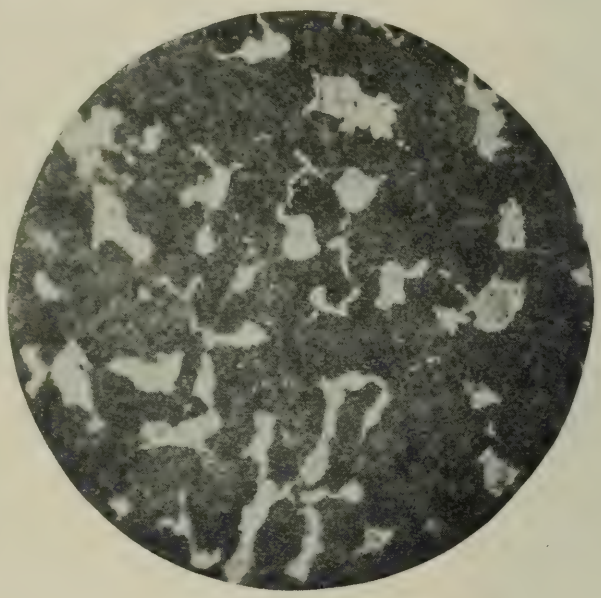
Alloy S 257 November 6th, 1914.

Composition—Ni, 0.35%; C, 0.060%; Fe, 99.6%.
Charge—S 254, 226 grams.
American Ingot Iron, 495 grams.
Nickel, 1.72 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment, same as S 250.
Weight of Casting—1 lb. 3 oz.
Note.—This alloy was cast twice before obtaining a satisfactory sample.

Alloy S 258

November 6th, 1914.

Composition—Ni, 0.50%; C, 0.072%; Fe, 99.4%.
Charge—American Ingot Iron, 935 grams.
Nickel, 4.67 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B 262.
Weight of Casting—1 lb. 5 oz.
Note—This alloy was cast three times before obtaining a satisfactory sample.

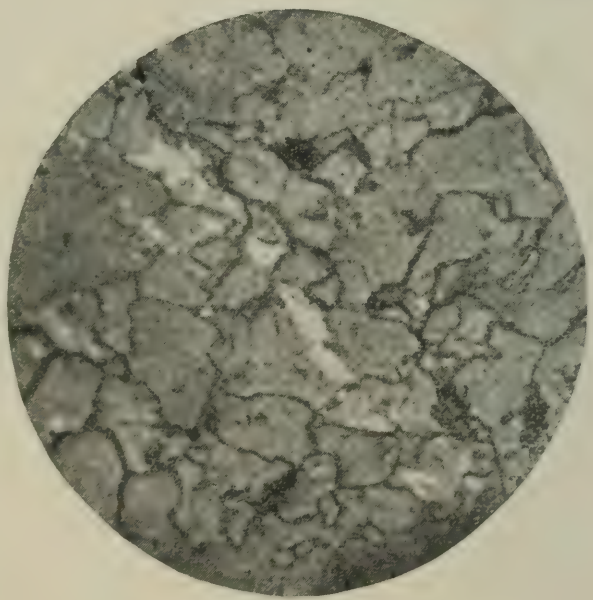


Alloy S 258.

Alloy S 259

November 9th, 1914.

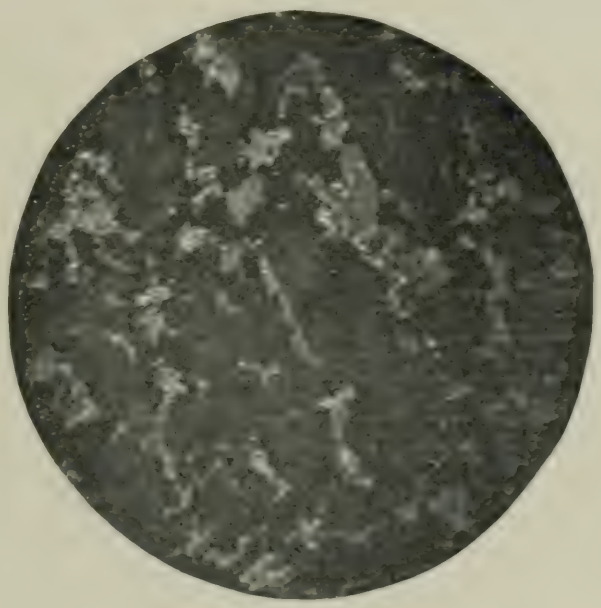
Composition—Ni, 0.75%; C, 0.067%; Fe, 99.2%.
Charge—S 258, 637 grams.
Nickel, 1.60 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as S 262.
Weight of Casting—1 lb. 5 oz.



Alloy S 259.

Alloy B_2 195 November 19th, 1914.

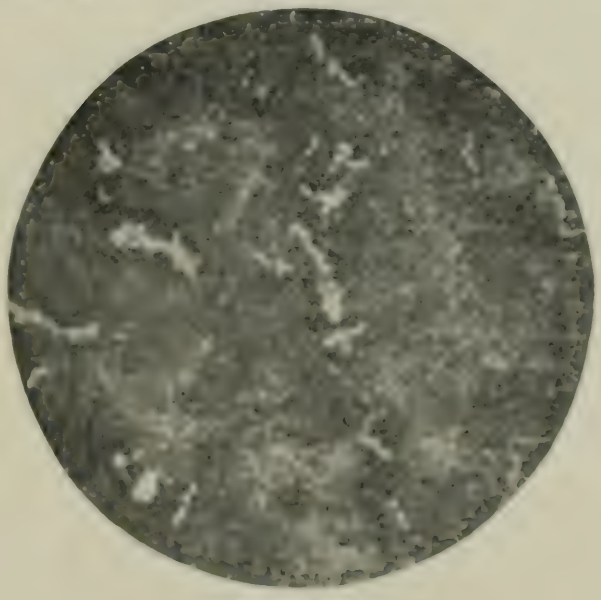
Composition — Ni, 1.0%; C, 0.089%; Fe, 98.9%.
Charge—S 261, 177 grams.
Nickel, 1.60 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B_2 199.
Weight of Casting—1 lb.



Alloy B_2 195.

Alloy S 263 November 10th, 1914.

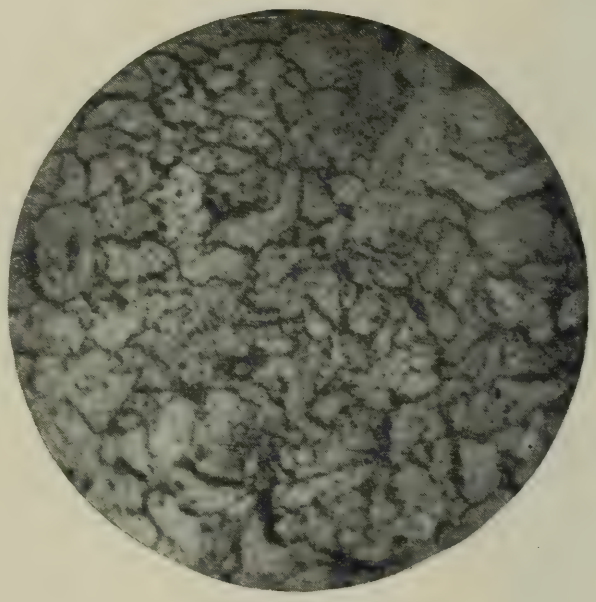
Composition—Ni, 1.0%; C, 0.065%; Fe, 98.9%.
Charge—S 261, 637 grams.
Nickel, 6.50 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B_2 199, with the exception that duration of soaking was 45 minutes.
Weight of Casting—1 lb. 3 oz.
Note.—This alloy was cast twice before obtaining a satisfactory sample.



Alloy S 263.

Alloy S 269 December 11th, 1914.

Composition—Ni, 2.0%; C, 0.085%; Fe, 97.9%.
Charge—American Ingot Iron, 566 grams.
Nickel, 11.58 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B_2 199, except soaking was 1 hour at 1700° C.
Weight of Casting—1 lb. 2 oz.



Alloy S 269.

Alloy S 267 December 10th, 1914.

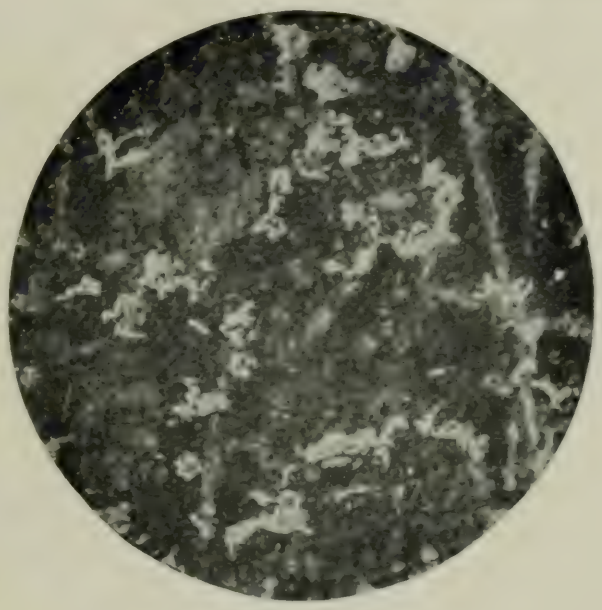
Composition—Ni, 0.25%; C, 0.23%; Fe, 99.5%.
Charge—C 204, 326 grams.
American Ingot Iron, 435 grams.
Nickel, 1.09 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B_2 199, except that soaking was 1 hour at 1700° C.
Weight of Casting—1 lb. 8 oz.

Alloy S 268 December 10th, 1914.

Composition—Ni, 0.50%; C, 0.21%; Fe, 99.2%.
Charge—B 30, 127 grams.
American Ingot Iron, 467 grams.
Nickel, 2.99 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B_2 199, except that soaking was 1 hour.
Weight of Casting—1 lb. 6 oz.

Alloy B 195 October 24th, 1914.

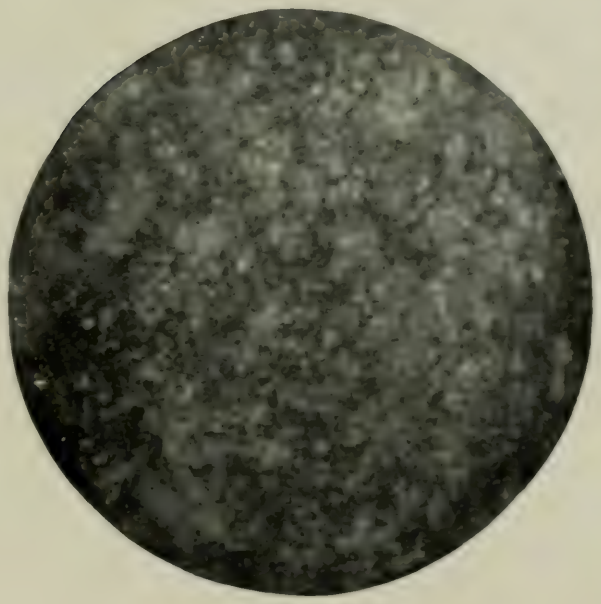
Composition — Ni, 1.0%; C, 0.24%; Fe, 98.8%.
Charge—B 207, 800 grams.
Nickel, 4.04 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B 202.
Weight of Casting—1 lb. 4 oz.



Alloy B 195.

Alloy B 196 October 27th, 1914.

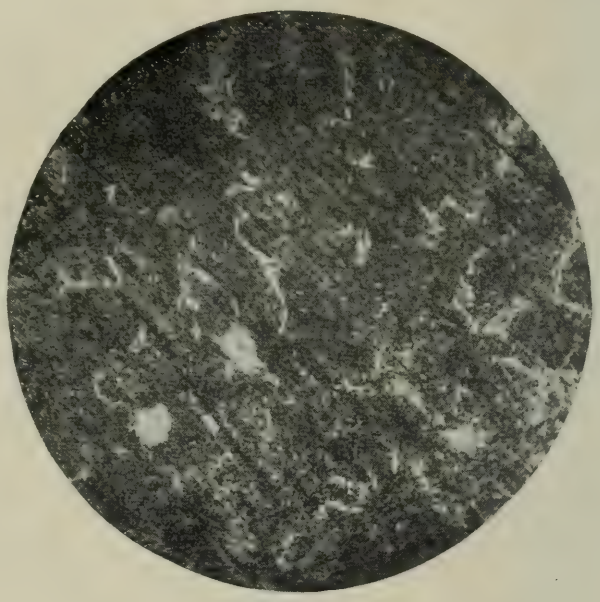
Composition — Ni, 2.0%; C, 0.23%; Fe, 97.8%.
Charge—B 195, 680 grams.
Nickel, 6.95 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B 202, except highest temperature was 1640° C.
Weight of Casting—1 lb. 6 oz.



Alloy B 196.

Alloy B_2 197 November 12th, 1914.

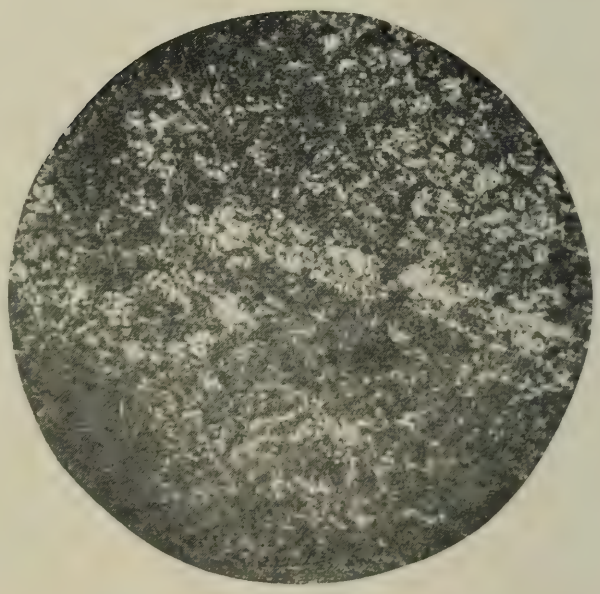
Composition — Ni, 3.0%; C, 0.13%; Fe, 96.8%.
Charge—S 263, 184 grams.
B 197, 368 grams.
Nickel, 1.90 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B_2 199.
Weight of Casting—8 oz.



Alloy B_2 197.

Alloy B 197 October 28th, 1914.

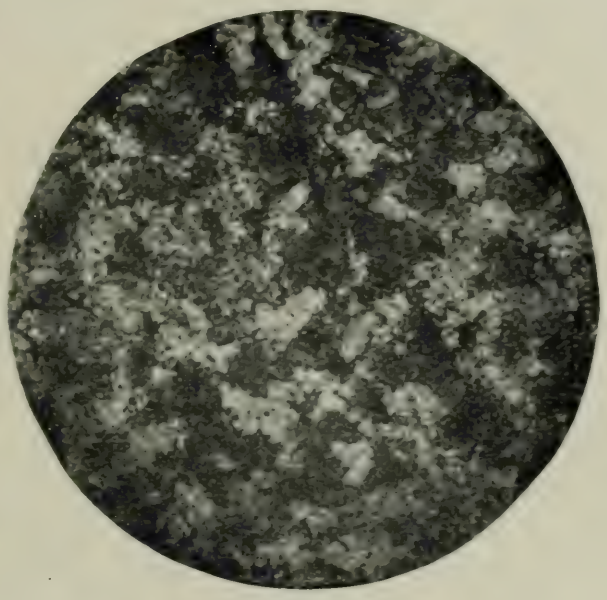
Composition — Ni, 3.0%; C, 0.21%; Fe, 96.7%.
Charge—B 196, 517 grams.
Nickel, 5.33 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B 202.
Weight of Casting—1 lb. 1 oz.



Alloy B 197.

Alloy C 204 November 25th, 1914.

Composition—Ni, 0.25%; C, 0.43%; Fe, 99.3%.
Charge—B 30, 269.5 grams.
American Ingot Iron, 269.5 grams.
Nickel, 1.35 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as S 250, except duration of soaking was 1 hour at 1560° C.
Weight of Casting—1 lb. 2 oz.

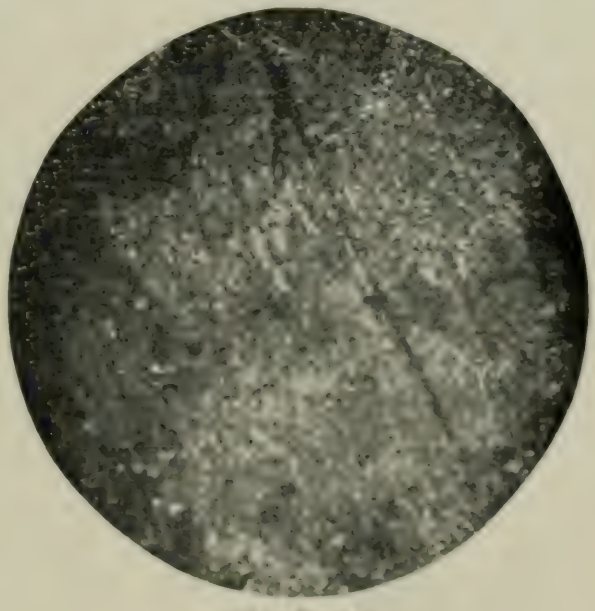


Alloy C 204.

Alloy B_2 205 November 16th, 1914.

Composition—Cu, 0.25%; C, 0.045%; Fe, 99.7%.
Charge—American Ingot Iron, 1062 grams.
Copper, 2.66 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B_2 199.
Weight of Casting—2 lb.

Note.—In this and the succeeding copper alloys the iron was first melted, and the copper dropped into the molten iron through a small hole in the furnace cover, the fall being directed by means of a tube of fused silica.



Alloy B_2 205.

Alloy B 205 October 28th, 1914.

Composition—Cu, 0.25%; C, 0.19%; Fe, 99.6%.

Charge—American Ingot Iron, 793 grams.
Copper, 0.25 gram.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B 202.

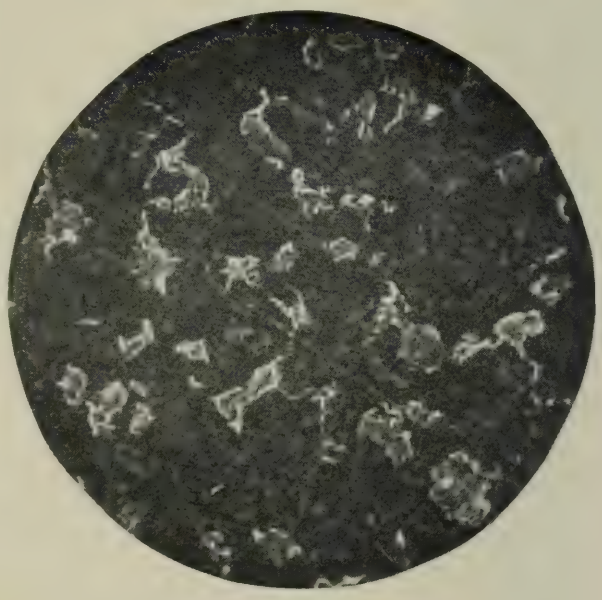
Weight of Casting—1 lb. 5 oz.

Alloy B_2 206 November 19th, 1914.

Composition—Cu, 0.50%; C, 0.17%; Fe, 99.3%.

Charge—B_2 205, 552 grams.
Copper, 1.39 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B 199.

Weight of Casting—1 lb. 2 oz.



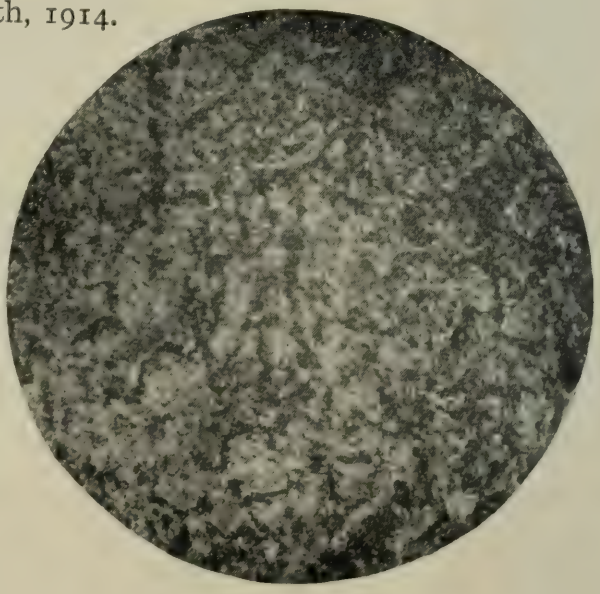
Alloy B_2 206.

Alloy B 206 October 30th, 1914.

Composition—Cu, 0.50%; C, 0.19; Fe, 99.3%.

Charge—B 205, 488 grams.
Copper, 1.23 grams.
Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B_2 199, except that highest temperature was 1640° C.

Weight of Casting—1 lb.



Alloy B 206.

Alloy B 208 November 24th, 1914.

Composition—Cu, 0.75%; C, 0.18%; Fe, 99.0%.

Charge — B_2 206, 318 grams.

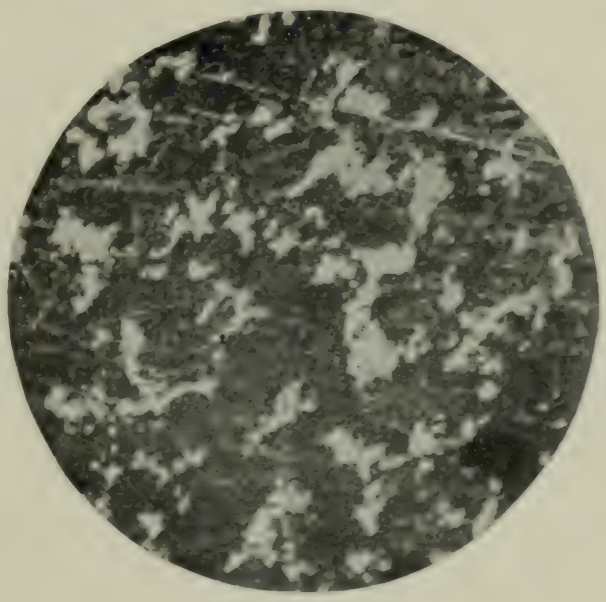
American Ingot Iron, 279.5 grams.

Copper, 4.40 grams.

Crucible, Furnace Treatment, Degasifier and Heat Treatment same as B_2 199.

Weight of Casting—1 lb. 2 oz.

Note.—This alloy was cast four times before obtaining a satisfactory sample.



Alloy B 208.

Samples were prepared from these alloys in the manner previously described and mounted for exposure on the roof of Nicol Building, Queen's University, Kingston, Ontario. (See page 275.) This series was mounted in the same way as Series II, and the duration of exposure was 253 days 3 hours, beginning December 22nd, 1914, 1.15 P.M., to December 23rd, 1914, 4.30 P.M., time ending September 1, 1915, 4.15 P.M.—September 2nd, 1915, 7.30 P.M. Total hours, 6075.

In addition to the samples prepared as above described, another set of samples was prepared from the American Rolling Mill Company alloys, and given a similar heat treatment.

After having been exposed the samples were brought in and carefully freed from rust and final measurements made as noted in the following table:

Series III

LOSS IN WEIGHT OF DISC

Sample Number of Alloy.	Approximate Analysis. Per Cent.	Loss of Weight in Grams Due to Corrosion.	Corrosion or Loss in Weight in Grams per Sq. Cm. of Original Surface per Hr. $\times 10^6$.	Remarks.
S 250 (*e*) (etched)	Fe 99.6 Co 0.25 C 0.083	0.5215	512	Rust light in color and fairly hard to remove.
B 202 (*a*)	Fe 99.5 Co 0.25 C 0.18	0.6279	635	Rust fairly light in color and easily removed.
B 202 (*b*)	.....	0.6663	604	
C 202 (*a*)	Fe 99.2 Co 0.25 C 0.49	0.4495	499	Rust medium dark in color and very tenacious.
C 202 (*b*)	.....	0.3921	498	
C 202(*e*) (etched)	.....	0.3589	484	
S 255 (*a*)	Fe 99.4 Co 0.35 C 0.21	0.5299	493	Rust light in color and fairly hard to remove.
S 255 (*b*)	.....	0.5026	475	
S 255 (*c*)	.....	0.4898	542	
S 255 (*e*)	.....	0.5773	534	
S 265 (*a*)	Fe 99.3 Co 0.35 Co 0.30	0.7654	659	Rust easily removed.
S 265 (*b*)	.....	0.7660	643	
S 265 (*c*)	.....	0.7712	649	
S 265 (*d*)	.....	0.6765	622	
C 255 (*a*)	Fe 99.2 Co 0.35 C 0.49	0.5264	562	Rust difficult to remove.
C 255 (*b*)	.....	0.4074	560	
C 255 (*e*)	.....	0.4094	585	

[7] Etched for microphotograph, see p. 46*b*.

SERIES III—*Continued*

LOSS IN WEIGHT OF DISC

Sample Number of Alloy.	Approximate Analysis. Per Cent.	Loss of Weight in Grams Due to Corrosion.	Corrosion or Loss in Weight in Grams per Sq. Cm. of Original Surface per Hr. $\times 10^3$.	Remarks.
B 209 (*a*)	Fe 99.2 Co 0.50 C 0.27	0.6530	607	Rust hard to remove.
B 209 (*b*)	.....	0.6032	863	
B 209 (*c*)	.....	0.6068	875	
S 252 (*a*)	Fe 99.1 Co 0.50 C 0.31	0.4686	479	Rust medium dark in color, tenacious.
S 252 (*b*)	.....	0.4454	487	
S 252 (*e*) (etched)	.....	0.4690	493	
S 260 (*a*)	Fe 99.1 Co 0.75 C 0.17	0.5345	500	Rust dark in color and quite difficult to remove.
S 260 (*b*)	.....	0.4822	502	
S 260 (*c*)	.....	0.5297	472	
S 260 (*d*)	.....	0.4761	527	
S 260 (*f*)	.....	0.4586	507	
S 260 (*g*)	.....	0.4819	492	
S 266 (*a*)	Fe 99.0 Co 0.75 C 0.21	0.2762	555	Rust quite dark and difficult to remove.
S 266 (*b*)	.....	0.4837	488	
S 266 (*c*)	.....	0.5229	530	
S 266 (*d*)	.....	0.5214	525	
B 198 (*a*)	Fe 98.6 Co 1.0 C 0.38	0.6047	615	Rust difficult to remove.
B 198 (*b*)	.....	0.5122	545	

Series III—*Continued*

LOSS IN WEIGHT OF DISC

Sample Number of Alloy.	Approximate Analysis. Per Cent.	Loss of Weight in Grams Due to Corrosion.	Corrosion or Loss in Weight in Grams per Sq. Cm. of Original Surface per Hr. $\times 10^8$.	Remarks.
B 198 (*e*)	.....	0.5246	539	
S 262 (*a*)	F 98.3 Co 1.0 C 0.62	0.3902	463	Rust dark in color and hard to remove.
S 262 (*e*)	.....	0.4719	514	
B 199 (*b*)	Fe 97.8 Co 2.0 C 0.16	0.6845	686	Rust coarse grained, easily removed.
B 199 (*e*)	.....	0.5391	495	
B 199 (*a*)	Fe 97.5 Co 2.0 C 0.46	0.6629	659	Rust fairly difficult to remove.
B 199 (*b*)	.....	0.4931	491	
B 199(*e*) (etched)	.....	0.5141	492	
B 199 (*c*)	.....	0.2171	433	
B 200 (*a*)	Fe 96.8 Co 3.0 C 0.17	0.4755	473	Rust medium dark in color; fairly coarse-grained.
B 200 (*b*)	.....	0.5138	476	
B 200(*e*) (etched)	.....	0.4866	448	
B 200 (*a*)	Fe 96.6 Co 3.0 C 0.36	0.3678	425	Rust dark in color, difficult to remove.
B 200 (*b*)	.....	0.2286	420	
B 200 (*e*)	.....	0.2830	446	Exposed 4839 hours.
S 251 (*a*)	Fe 99.6 Ni 0.25 C 0.057	0.7074	585	Rust very loose.
S 251 (*b*)	.....	0.6010	578	
S 251 (*e*)	.....	0.7245	592	

SERIES III—*Continued*

LOSS IN WEIGHT OF DISC

Sample Number of Alloy.	Approximate Analysis. Per Cent.	Loss of Weight in Grams Due to Corrosion.	Corrosion or Loss in Weight in Grams per Sq. Cm. of Original Surface per Hr. $\times 10^3$.	Remarks.
B 204 (*a*)	Fe 99.6 Ni 0.25 C 0.10	0.6297	578	Rust coarse-grained; easily removed.
B 204 (*b*)	.....	0.5356	585	
S 267 (*a*)	Fe 99.5 Ni 0.25 C 0.23	0.6007	637	Rust dark in color and difficult to remove.
S 267 (*b*)	.....	0.5323	598	
S 267 (*c*)	.....	0.5689	563	
S 267 (*d*)	Fe 99.5 Ni 0.25 C 0.23	0.5832	588	Rust dark in color and difficult to remove.
C 204 (*a*)	Fe 99.3 Ni 0.25 C 0.43	0.5018	522	Rust fairly loose and light in color.
C 204 (*b*)	.....	0.5483	502	
C 204 (*c*)	.....	0.5670	502	
S 254 (*a*)	Fe 99.6 Ni 0.35 C 0.045	0.5707	555	Rust coarse-grained on surface, but fine-grained below.
S 254 (*b*)	.....	0.6189	623	
S 254 (*e*)	.....	0.6202	623	
S 257 (*a*)	Fe 99.6 Ni 0.35 C 0.060	0.5138	533	Rust medium in color and quite tenacious.
S 257 (*b*)	.....	0.5218	531	
S 258 (*a*)	Fe 99.4 Ni 0.50 C 0.072	0.4522	451	Rust very dark in color and removed with great difficulty.
S 258 (*e*)	.....	0.4798	442	
S 268 (*a*)	Fe 99.2 Ni 0.50 C 0.21	0.4173	489	Rust medium dark in color and quite fine-grained.

SERIES III—*Continued*

LOSS IN WEIGHT OF DISC

Sample Number of Alloy.	Approximate Analysis. Per Cent	Loss of Weight in Grams Due to Corrosion.	Corrosion or Loss in Weight in Grams per Sq. Cm. of Original Surface per Hr. $\times 10^3$	Remarks.
S 268 (*b*)	.....	0.3074	476	
S 268 (*e*)	.....	0.4670	557	Exposed 4896 hours.
S 259 (*a*)	Fe 99.2 Ni 0.75 C 0.067	0.5830	533	Rust fairly loose and light in color.
S 259 (*b*)	.....	0.5518	547	
S 259 (*c*)	.....	0.5784	565	
S 259 (*d*)	.....	0.5243	533	
S 259(*e*) (etched)	.....	0.5917	525	
S 263 (*e*)	Fe 98.9 Ni 1.0 C 0.065	0.4574	455	Rust dark and very hard to remove.
B_2 195 (*a*)	Fe 98.9 Ni 1.00 C 0.089	0.4998	509	Rust dark in color and hard to remove.
B_2 195 (*b*)	.....	0.5310	495	
B_2 195 (*e*)	.....	0.5356	500	
B 195 (*b*)	Fe 98.8 Ni 1.0 C 0.24	0.4239	470	Rust very dark in color and difficult to remove.
B 195 (*e*)	.....	0.4917	462	
S 269 (*a*)	Fe 97.9 Ni 2.0 C 0.085	0.4893	488	Rust dark in color, coarse-grained and hard to remove.
S 269 (*b*)	.....	0.5175	470	
S 269 (*e*)	.....	0.4936	538	4896 hours exposed.
B 196 (*a*)	Fe 97.8 Ni 2.0 C 0.23	0.5101	452	Rust dark, fine-grained and quite tenacious.
B 196 (*b*)	.....	0.5065	447	

SERIES III—*Continued*

LOSS IN WEIGHT OF DISC

Sample Number of Alloy.	Approximate Analysis. Per Cent.	Loss of Weight in Grams Due to Corrosion.	Corrosion or Loss in Weight in Grams per Sq. Cm. of Original Surface per Hr. $\times 10^3$.	Remarks.
B 196 (*e*)	.....	0.4074	495	
B_2 197 (*a*)	Fe 96.8 Ni 3.0 C 0.13	0.4374	429	Rust very dark and fine-grained; quite tenacious.
B_2 197 (*b*)	.....	0.4276	446	
B_2 197 (*c*)	.....	0.3830	438	
B 197 (*a*)	Fe 96.7 Ni 3.0 C 0.21	0.4093	398	Rust very dark in color.
B 197 (*b*)	.....	0.3621	386	
B 197 (*e*)	.....	0.3666	379	
B_2 205 (*a*)	Fe 99.7 Cu 0.25 C 0.045	0.5586	663	Rust very loose and light in color.
B_2 205 (*b*)	.....	0.6646	683	
B_2 205 (*c*)	.....	0.6211	712	
B_2 205 (*d*)	.....	0.7335	701	
B 205 (*a*)	Fe 99.6 Cu 0.25 C 0.19	0.5222	482	Rust fairly dark in color and tenacious.
B 205 (*b*)	.....	0.5256	478	
B 205 (*e*)	.....	0.4517	541	Exposed 4896 hours.
B_2 206 (*a*)	Fe 99.3 Cu 0.50 C 0.17	0.4500	440	Rust light in color and difficult to remove.
B_2 206 (*b*)	.....	0.4500	443	
B_2 206 (*e*)	.....	0.4689	443	
B 206 (*a*)	Fe 99.3 Cu 0.50 C 0.19	0.5153	482	Rust light in color and loose.

SERIES III—*Continued*

LOSS IN WEIGHT OF DISC

Sample Number of Alloy.	Approximate Analysis. Per Cent.	Loss of Weight in Grams Due to Corrosion.	Corrosion or Loss in Weight in Grams per Sq. Cm. of Original Surface per Hr. $\times 10^8$.	Remarks.
B 206 (*b*)	.....	0.5501	478	
B 206 (*e*)	.....	0.5520	456	
B 208 (*a*)	Fe 99.0 Cu 0.75 C 0.18	0.4575	519	Rust dark in color and fairly tenacious.
B 208 (*b*)	.....	0.4160	497	
B 208 (*e*)	.....	0.5534	492	
34204 (*a*)	American Ingot Iron 99.6 Co 0.35 C 0.01	0.7672	638	Rust light in color and fairly tenacious. Exposed 5043 hours.
34204 (*b*)	.....	0.6925	643	
[8] 34204 (*c*)	.....	0.7078	638	
34204 (*d*)	.....	0.7568	638	
34196 (*a*)	American Ingot Iron 99.3 Co 0.60 C 0.01	0.9175	640	Rust light in color, scaly.
34196 (*b*)	.....	0.8412	582	
34196 (*c*)	.....	0.9256	613	
34196 (*d*)	.....	0.9991	706	
34196 (*f*)	.....	0.8939	624	
34185 (*a*)	American Ingot Iron 98.8 Co 1.18 C 0.01	0.7916	522	Rust medium in color and fairly difficult to remove. Exposed 6018 hours; found on roof.

[8] These samples were mounted February 4, 1915, at 2:35 P.M.

SERIES III—*Continued*

LOSS IN WEIGHT OF DISC

Sample Number of Alloy.	Approximate Analysis. Per Cent.	Loss of Weight in Grams Due to Corrosion.	Corrosion or Loss in Weight in Grams per Sq. Cm. of Original Surface per Hr. $\times 10^6$.	Remarks.
34185 (*b*)	.....	0.8270	533	
34185 (*c*)	.....	0.8301	532	
34185 (*d*)	.....	0.8279	535	

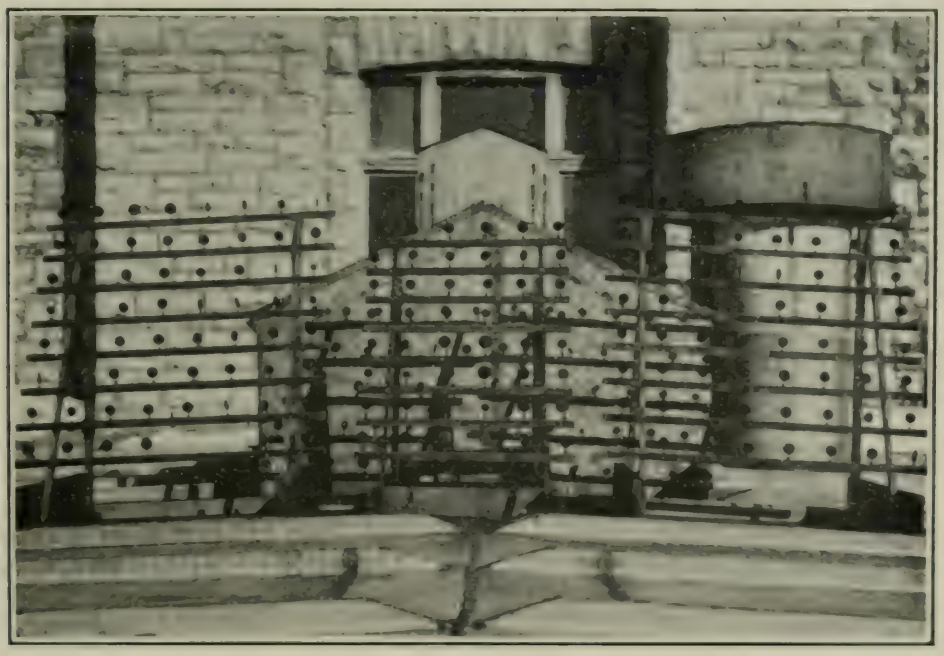
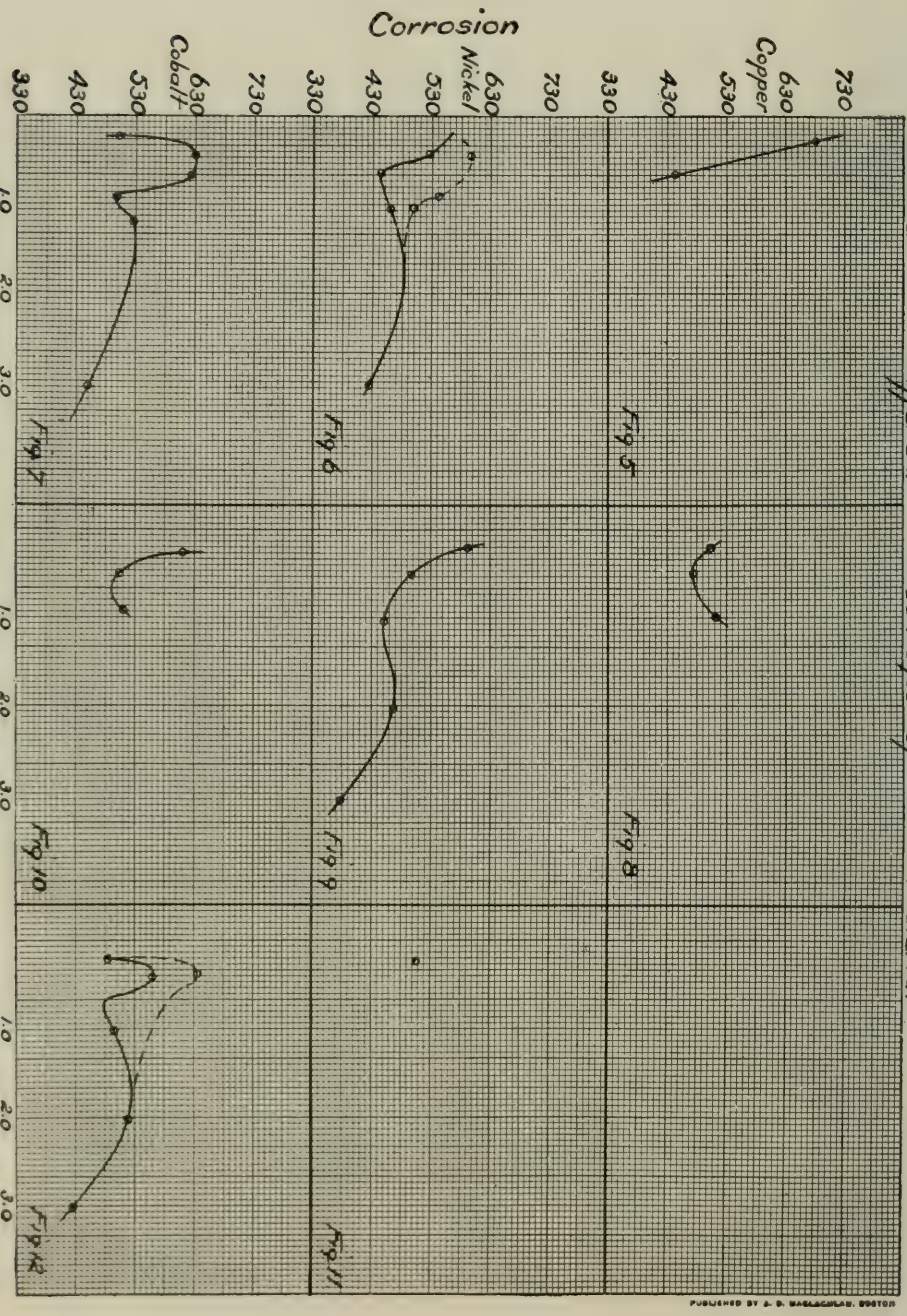
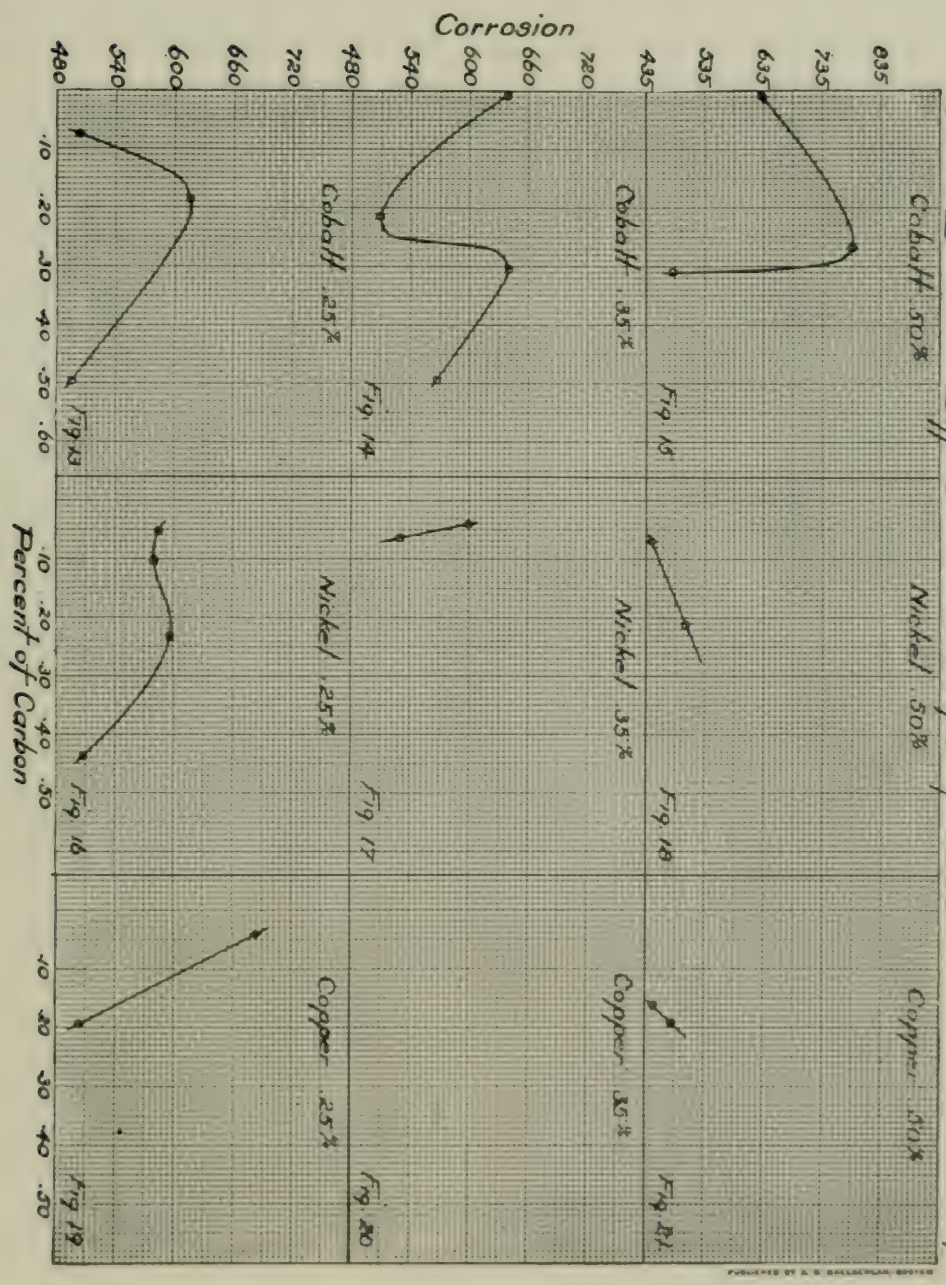
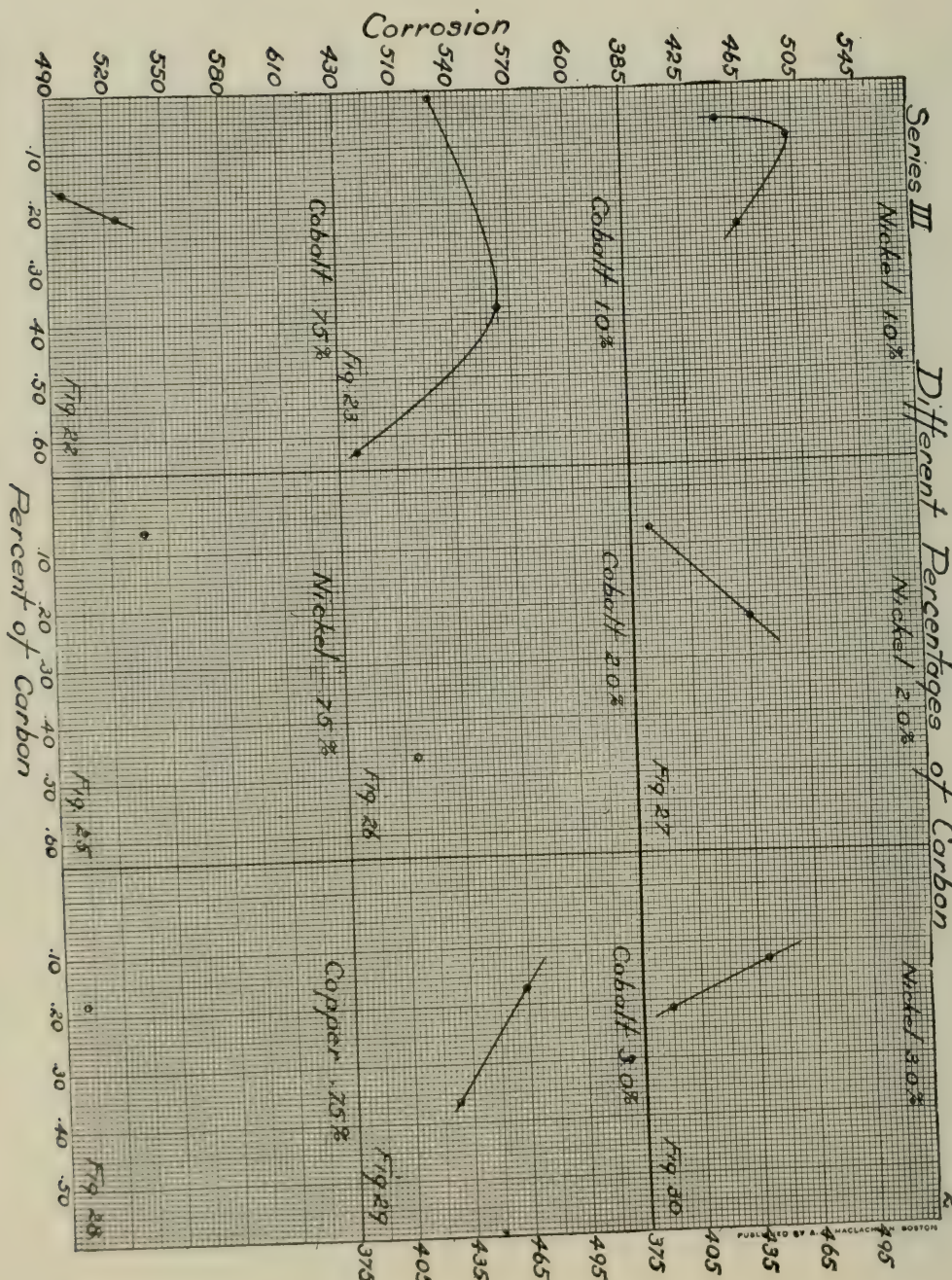


FIG. 1A.


Series III Different Percentages of Same Material
Corrosion
Copper
Nickel
Cobalt
730
630
530
430
330
1.0
2.0
3.0
Fig 5
Fig 6
Fig 7
Fig 8
Fig 9
Fig 10
Fig 11
Fig 12
Low Carbon
Medium Carbon
High Carbon


Series III
Different Percentages of Carbon
Corrosion
Percent of Carbon
Cobalt .50%
Nickel .50%
Copper .50%
Cobalt .35%
Nickel .35%
Copper .35%
Cobalt .25%
Nickel .25%
Copper .25%
Fig 13
Fig 14
Fig 15
Fig 16
Fig 17
Fig 18
Fig 19
Fig 20
Fig 21


Series III
Different Percentages of Carbon
Corrosion
Percent of Carbon
Nickel 10%
Cobalt 10%
Cobalt 7.5%
Fig 23
Fig 24
Nickel 2.0%
Cobalt 2.0%
Fig 27
Nickel 7.5%
Fig 26
Fig 25
Nickel 3.0%
Cobalt 3.0%
Fig 30
Copper .25%
Fig 29
Fig 28
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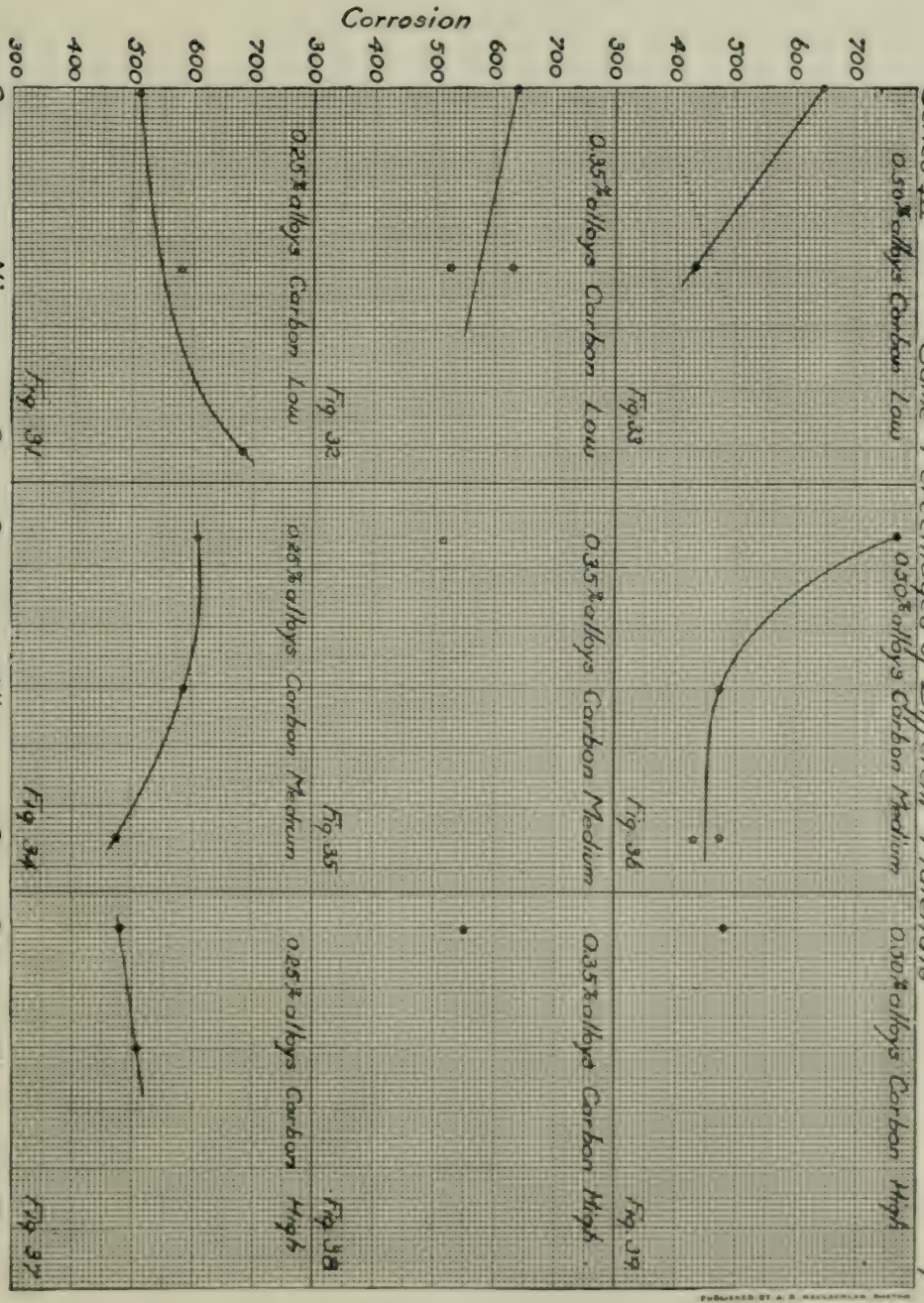
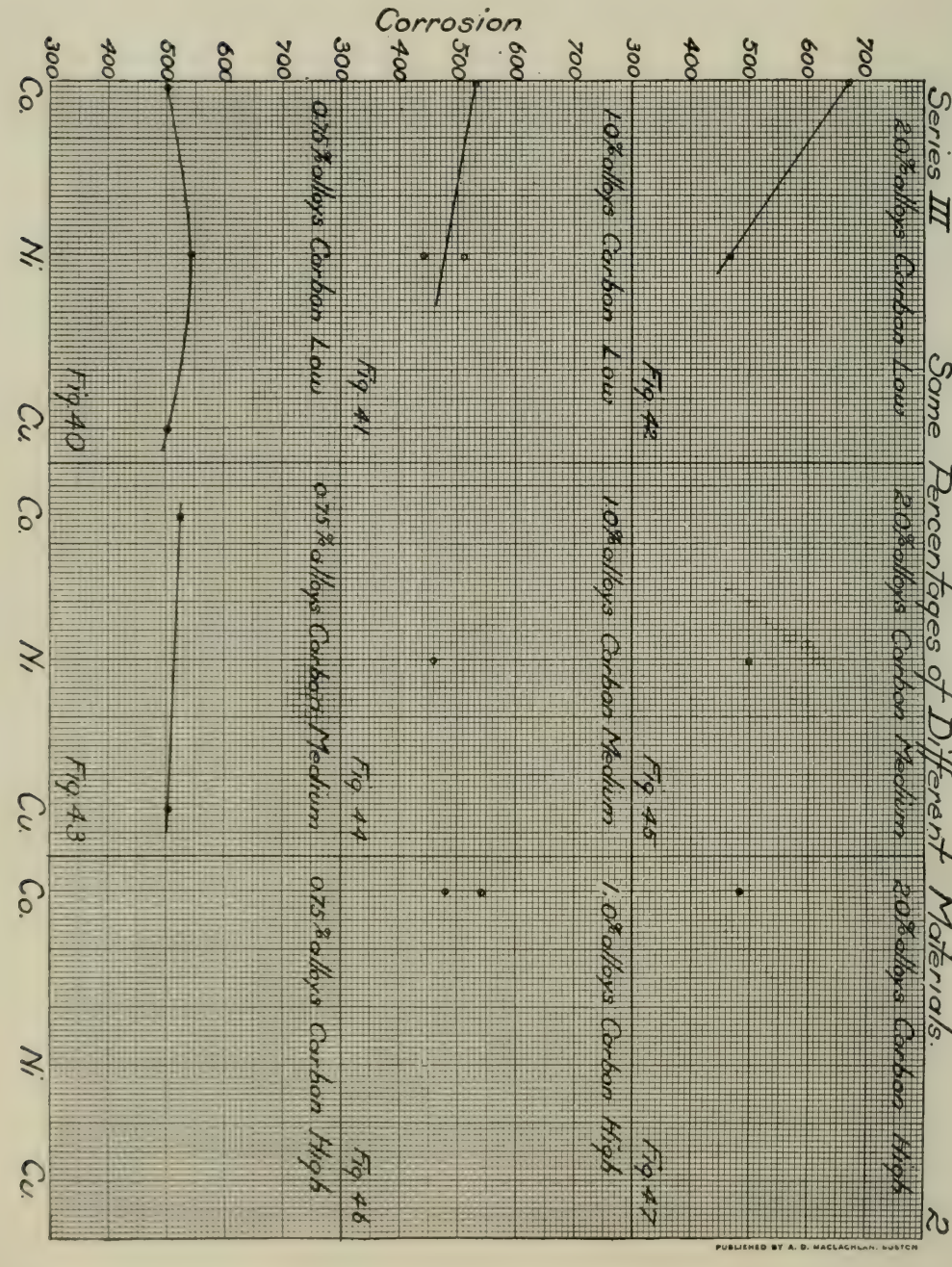

Series III. Same Percentages of Different Materials
Corrosion
300
400
500
600
700
0.10% alloys Carbon Low
0.35% alloys Carbon Low
0.25% alloys Carbon Low
0.10% alloys Carbon Medium
0.35% alloys Carbon Medium
0.25% alloys Carbon Medium
0.10% alloys Carbon High
0.35% alloys Carbon High
0.25% alloys Carbon High
Fig. 31
Fig. 32
Fig. 33
Fig. 34
Fig. 35
Fig. 36
Fig. 37
Fig. 38
Fig. 39
Co.
Ni
Cu
Co.
Ni.
Cu
Co.
Ni.
Cu


Series III Some Percentages of Different Materials. 2
Corrosion
300
400
500
600
700
Co. Ni Cu. Co. Ni Cu. Co. Ni Cu.
2.0% alloys Carbon Low
2.0% alloys Carbon Medium
2.0% alloys Carbon High
1.0% alloys Carbon Low
1.0% alloys Carbon Medium
1.0% alloys Carbon High
0.25% alloys Carbon Low
0.75% alloys Carbon Medium
0.75% alloys Carbon High
Fig 40
Fig 41
Fig 42
Fig 43
Fig 44
Fig 45
Fig 46
Fig 47
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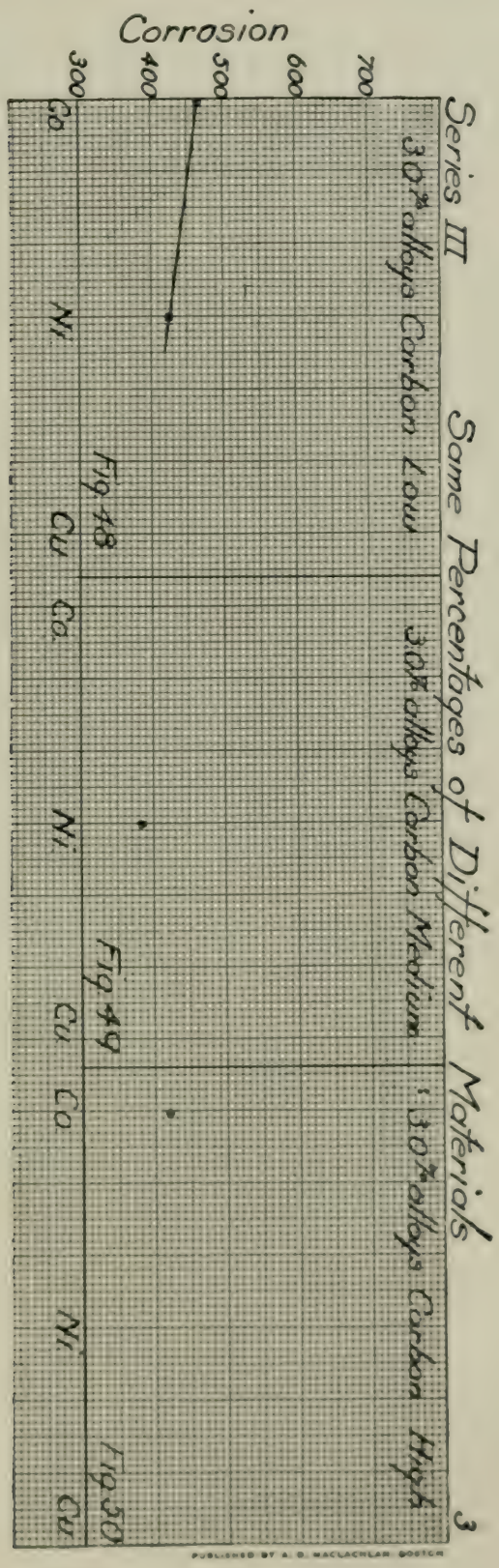
Corrosion
300
400
500
600
700
Series III
Same Percentages of Different Materials
3.0% alloys Carbon Low
3.0% alloys Carbon Medium
3.0% alloys Carbon High
Co
Ni
Fig 48
Cu
Co
Ni
Fig 49
Cu
Co
Ni
Fig 50
Cu
3
PUBLISHED BY A. D. MACLACHLAN BOSTON

Series IV

Accelerated Corrosion Tests

A few accelerated corrosion tests were made on some of these alloys, the results of which may be instructive, although not conclusive. The authors do not believe that much reliance can be placed upon conclusions as to what would occur under service conditions in the atmosphere, which are drawn from accelerated tests. These measurements are included with the rest for completeness.

Accelerated corrosion tests were made on the five heats of Ingot Iron alloys submitted by the American Rolling Mill Company as follows:

No. 34175

S	0.026	Mn	0.022
P	0.009	Cu	0.016
C	0.010		

No. 34185

S	0.034	Mn	0.017
P	0.006	Cu	0.028
C	0.015	Co	1.18

No. 34196

S	0.040	Mn	0.020
P	0.008	Cu	0.024
C	0.010	Co	0.60

No. 34204

S	0.022	Mn	0.020
P	0.009	Cu	0.020
C	0.010	Co	0.35

No. 44009

S	0.025	Mn	0.015
P	0.008	Cu	0.24
C	0.010	Ni	0.75

Method of Making Accelerated Corrosion Tests

These tests were made either by (*a*) immersing the samples in the form of spheres in a dilute sulphuric acid for a period of 1 hour or noting the loss in weight, or, (*b*) submitting them to the intermittent action of dilute sulphuric acid and the atmosphere. The acid used for the tests was 20% H_2SO_4.

Samples were used throughout in the form of spheres of approximately 7 sq. cms. surface.

The intermittent corrosion tests were made by immersing the samples in the acid in one compartment of a wooden box, so arranged that the samples were covered and uncovered automatically by the tilting of this box over a knife edge pivot by the action of water from the water tap. The balls were held in place at one end of the box by pieces of glass rod.

The apparatus used for making these corrosion tests consisted of a wooden box about 18″ long and 12″ wide, with slides about 3″ high. This box was divided into 2 lengthwise compartments, one larger than the other, and the larger one being still further divided into 2 equal compartments, this division being across the narrow way of the box directly in the middle, the dividing board extending well above the edge of the box. Across the outside of the bottom of the box in the middle a wooden knife edge was fastened.

The operation was somewhat as follows: The samples were placed in one end of the undivided lengthwise compartment, supported and held in place by glass rods, and covered with a corroding solution. This unbalanced the box so that it inclined to one side, keeping the samples under the solution until the box was tilted to the opposite side. This tilting was accomplished by opening the water gap above the corrosion box, which allowed the stream of water to run into the high side of the large divided compartment, this being the end opposite to the one in which the samples were placed. As soon as this compartment was sufficiently full of water to overbalance the weight of the corroding solution and the samples, the box tilted, causing the solution to flow away from the samples and at the same time directing a stream of water from the tap, by means of the extended dividing board, into the other compartment, which again tended to overbalance the box and cause it to assume its original position.

In the end of each of the water compartments, just above the water line or the line to which it was necessary for the water to

rise and overbalance the box, a hole was made which allowed the water to run out after it had been overbalanced. On account of the extra weight of the samples and glass rods it was necessary to have this water line fairly high, thus requiring an automatic syphon, consisting of a bent glass tube inserted into the hole and touching the bottom of the box, to completely drain. The action of the box was made eccentric by attaching to one side of the water-diverting board a small trough which carried away into the sink most of the water flowing from the tap on that side. The compartment on this side of the box was filled by the water, which dropped through holes made in the trough for this purpose. The stream from the tap and these holes were so adjusted that a complete period of the box was about half an hour.

Accelerated Corrosion Measurements

May 21st, 1914, the average dimension of the spheres before corrosion was found to be as follows:

Sample Number.	Diameter in Cms.	Surface in Sq. Cms.	Weight in Grams before Corrosion.	Final Weight in Grams.	Loss in Weight in Grams.	Loss in Weight per Sq. Cm. of Exposed Surface per Hour.
34,175	1.2656	7.952	8.3428	8.3273	0.0155	0.00216
34,185	1.2426	7.808	7.8775	7.8608	0.0167	0.00238
34,196	1.1811	7.421	6.7720	6.7584	0.0136	0.00204
34,204	1.1739	7.376	6.6582	6.6440	0.0142	0.00214
44,009	1.1928	6.867	5.3586	5.3514	0.0072	0.00117

During the course of these experiments the 5 spheres were each immersed in the acid for 54 minutes, and exposed to the air within the room of the laboratory for 5 hours and 5 minutes. This immersion and exposure were each divided into 12 approximately equal intervals, the twelve 4½ minute immersions with the corresponding twelve exposures constituting the corrosion test.

In columns 5 and 6 of the above table are given the final weight in grams after the corrosion test and the corresponding loss in weight.

A check was run of the above corrosion test as indicated in the following table (May 26th, 1914):

Sample Number.	Diameter in Cms.	Surface in Sq. Cms.	Weight in Grams before Corrosion.	Final Weight in Grams.	Loss in Weight in Grams.	Loss in Weight per Sq. Cm. of Exposed Surface per Hour.
34,175	.....	7.953	8.3199	8.3033	0.0166	0.00232
34,185	.....	7.808	7.8532	7.8352	0.0180	0.00256
34,196	.....	7.421	6.7530	6.7386	0.0144	0.00216
34,204	.....	7.376	6.6374	6.6222	0.0152	0.00229
44,009	.....	6.867	5.3483	5.3404	0.0079	0.00216

It will be seen from the above tables that the two sets of measurements agree very well among themselves and that the order of passivity of the alloys is as follows:

44009 34196 34204 34175 34185

For comparison of the above the standard sulphuric acid accelerated corrosion test was run with the modified one just described. These tests were made by immersing the 5 samples above described in 20% sulphuric acid continuously for 54 minutes. The following table gives the results:

Sample Number.	Weight in Grams before Immersion.	Weight in Grams after Immersion.	Loss in Weight in Grams.	Loss in Weight in Grams per Sq. Cm. of Exposed Surface per Hour.
34,175	8.3273	8.3235	0.0038	0.00053
34,185	7.8608	7.8569	0.0039	0.00055
34,196	6.7584	6.7557	00.027	0.00040
34,204	6.6440	6.6406	0.0034	0.00051
44,009	5.3514	5.3498	0.0016	0.00026

A check run on this corrosion test identical with the above was made with the following results:

34,175	8.3235	8.3199	0.0036	0.00052
34,185	7.8569	7.8532	0.0037	0.00053
34,196	6.7557	6.7530	0.0027	0.00040
34,204	6.6406	6.6374	0.0032	0.00050
44,009	5.3498	5.3483	0.0015	0.00024

These two tests agree with one another and show the order of passivity of the alloys to be as follows:

44009 34196 34204 34175 34185

Conclusion

If these accelerated corrosion tests could be relied upon as accurately reproducing atmospheric conditions, it would be clear that the addition of monel metal to American ingot iron to the extent of about 1% produces a more non-corrosive alloy for sheet roofing materials than the addition of similar small percentages of cobalt.

This type of corrosion test shows the alloy containing 0.60% Co to be more non-corrosive than either the one containing more (1.18% Co) or the one containing less (0.35% Co).

CORROSION TESTS, AMERICAN INGOT IRON ALLOYS IN THE FORM OF SHEET-ROOFING MATERIALS

In addition to the numerous corrosion experiments on discs of the alloy above mentioned, there were atmospheric corrosion tests made on sheets of these alloys under service conditions.

Pure cobalt of the analysis shown on page 6 was sent from this Laboratory to the American Rolling Mill Company, Middletown, Ohio, early in 1913. It was arranged to make up charges in their four-ton experimental furnace, such as could be rolled into full-sized sheets to be subjected to the usual corrosion tests. Three such cobalt alloy sheets were sent to this laboratory, together with a run similarly prepared, using monel metal in place of cobalt and a sheet of standard American Ingot Iron for comparison. These sheets were 30″ by 96″ in dimensions and No. 26 gage 0.0188″ in thickness. All of these sheets were received in duplicate.

Analyses

Following are the analyses of the above-mentioned sheets:

American Ingot Iron 34,175.		1 Per Cent Co Alloy 34,185.		0.60 Per Cent Co Alloy 34,196.		0.35 Per Cent Co Alloy 34,204.		1 Per Cent Monel Metal 44,099.	
S	0.026	S	0.034	S	0.040	S	0.022	S	0.025
P	0.009	P	0.006	P	0.008	P	0.009	P	0.008
C	0.010	C	0.015	C	0.010	C	0.010	C	0.010
Mn	0.022	Mn	0.017	Mn	0.020	Mn	0.020	Mn	0.015
Cu	0.016	Cu	0.028	Cu	0.024	Cu	0.020	Cu	0.24
		Co	1.18	Co	0.600	Co	0.35	Ni	0.75

These five sheets were mounted side by side on a wooden frame built to support them. They were exposed in a plane making 60 degrees with a horizontal. They have been corroding since March 18th, 1914, and since that time photographic and visual observations have been made at regular intervals.

It will take at least another year for these sheets to corrode through to destruction, before which time no final conclusions can be drawn. Up to this time it would appear, however, that the loss of weight of the 1% cobalt alloy and of the 0.60% cobalt alloy was less than that of the others. Both of these have formed a very hard, dense, tenacious, protective coating.

While there may be some doubt as to which is the least corroded sheet of the three, the 1% cobalt alloy, the 0.60% cobalt alloy, or the 1% monel metal alloy, there can be no doubt as to the fact that all of these three are far superior in non-corrosive action to the sheet of pure American Ingot Iron, or to the sheet containing 0.35% cobalt alloy. The pure American Ingot Iron sheet is the most corroded to date.

Following are figures 1 to 50, these being graphs for the purpose of visualizing the foregoing data. From the data and graphs the following general conclusions may be drawn:

General Conclusions

1. The corrosion, or loss of weight in grams per square centimeter of original surface per hour, is the function of the length of exposure, being less for the longer exposures. This is true because of the property of these alloys to form a self-protecting layer or coating.

2. The graphs representing Series I and II show remarkable similarity of form. These series were for independent corrosions of the same set of samples. Therefore such irregularities as appear in general are probably due not to uncertainties of measurement but to lack of control of the physical structure of the alloys in preparation.

3. The alloys formed by the addition of small percentages of copper, nickel and cobalt (from 0.25% to 3.0%) to American Ingot Iron, are more resistant to atmospheric corrosion than the pure American Ingot Iron, from which the alloys were prepared.

4. Considering the data for alloys formed by adding various amounts of cobalt (from 0.25% to 3.0%) to American Ingot Iron,

with very little, if any, carbon content, it is apparent that the corrosion is not a simple function of the percentage of cobalt content. In general, the corrosion of the alloys formed by the addition of 3% of cobalt to American Ingot Iron is about 75%, as great as that of the alloys formed by the addition of 0.5% of cobalt.

5. Alloys formed by the addition of 0.25% to 3.0% cobalt to American Ingot Iron, with very little, if any, carbon content, are corroded in the atmosphere to an extent varying between 50% and 75% of that of the pure American Ingot Iron from which the alloys were prepared.

6. Conclusions 4 and 5 are approximately true for the corresponding nickel alloys as for the cobalt alloys. There seems to be very little choice between the use of nickel and cobalt to form alloys with American Ingot Iron containing between 0.25% and 3.0% of the added metal for the prevention of corrosion. This is true so far as the disc tests show, but with the exception noted in conclusion 7.

7. As corrosion progresses, all of the alloys prepared form self-protective coatings of oxides. It is noticeable throughout that the oxides formed by the cobalt are darker, denser, and more tenacious than those formed by the other alloys.

8. The effect of preventing corrosion of the protective coating mentioned in conclusion 7 does not seem to have worked greatly to the advantage of cobalt alloys, in spite of its more satisfactory appearance, in the length of time that the above experiments were allowed to run.

9. In order to finally conclude the possible ultimate advantages of the cobalt alloy protective coating, as compared with the other sheets, all of the alloys should be allowed to corrode to destruction. The results of such tests, as discussed in the text above, will be published later.

10. The addition of copper to American Ingot Iron to an extent between 0.25% and 0.75% seems to be conducive to reducing the corrosion of American Ingot Iron under atmospheric conditions. It is difficult to say whether or not the addition of copper in these amounts has a greater or lesser effect than the corresponding amounts of nickel or cobalt. Additional experiments will be required to determine these facts, but there can be but little doubt that the addition of copper, as above reported, diminishes the corrosion of the pure American Ingot Iron.

11. The amount of corrosion varies with the percentage of carbon in the alloy, as would be expected, and as may best be seen by reference to the graphs.

During the course of making the large number of observations set forth in this paper, which extended over a period of several years, the authors were from time to time assisted by Mr. C. H. Harper, Research Laboratory, Queen's University, Kingston, Ontario, now Professor, Moosejaw College, Saskatchewan; and Mr. Walter L. Savell, Research Laboratory, Queen's University, Kingston, Ontario, now Metals Department, Deloro Mining & Reduction Company, Deloro, Ontario.

The authors wish hereby to acknowledge their indebtedness to these gentlemen, and as well to Mr. R. C. Wilcox, Research Laboratory, Queen's University, Kingston, Ontario, now analyst, The Exolon Company, Thorold, Ontario, by whom most of the analyses reported in this paper were made.

DISCUSSION

Dr. A. S. Cushman: Mr. Chairman and Gentlemen of the Institute.—It is, of course, natural that I should be much interested in any scientific paper that sets forth a way of improving the product discussed in this paper, which is known as American Ingot Iron. American Ingot Iron is a product which has been developed in the United States in the last decade, and is claimed to be and is the purest commercial iron ever made in large tonnage quantities and offered to the public on a guaranteed analysis of purity. It was developed by the American Rolling Mill Co.

Before I entered independent practice, and was engaged in the U. S. Department of Agriculture, in making certain investigations on behalf of the Government, I became convinced that much of the commercial steel made in America was rusting very much faster than there was need for. This conclusion applied not only to wire, but to sheet products, which, of course, are the two types of iron and steel that are most vulnerable when it comes to corrosion, because they are used in such fashion and for such purposes that the sections are very thin as compared with material like bridge members or railroad rails, or any other large construction steel, which by its very bulk and thickness insures itself considerable longevity. But when

iron and steel is rolled into thin sections, as for instance, into wire for wire fencing, or into sheets for roofing or siding, or even to go to the slightly thicker sections known as plates, corrosion becomes a very important problem, so important indeed, that it has engaged the attention of the meetings of so many different kinds of learned societies and technical societies that it is almost impossible to find a program to-day set before any body of technical chemists, engineers or manufacturers that will not contain at least one paper concerning the corrosion of iron and steel.

My earlier researches convinced me that the imported pure irons from Norway and Sweden deserve the high reputation that they had earned with regard to resistance to corrosion. The results of tests also pointed toward high purity as an important, if not the most important, factor in a slow-rusting metal. Norwegian and Swedish metallurgical processes were not adapted to the economic conditions prevailing in this country, and it was apparent that if commercially pure iron was to be manufactured it would have to be made in an open hearth furnace on the same scale of operation used in steel making. I believed this could be done, although I fully realized that a great many difficulties would have to be overcome, possibly involving new discoveries.

Distinguished technical journals ridiculed my early suggestions and stated editorially that the attempt to eliminate manganese in the open hearth furnace would result in a worthless product, if not the destruction of the furnace. Metallurgical authorities in Germany, Sweden and the United States had incorporated similar statements in their textbooks. I have a cherished letter in my files from a celebrated American steel man stating that it was impossible to make metal in an open hearth furnace to contain less than 0.2 per cent manganese. Curiously enough, as I thought at the time, my suggestions aroused bitter hostility and even ridicule from almost every quarter where they received any consideration at all. The one exception in the beginning, however, was the American Rolling Mill Co., to whom reference has been freely made in the paper under discussion. It must, I think, be conceded that it required faith and courage of a high order to undertake plant research work on a scale that involves many experimental heats of sixty tons of molten metal and the possible destruction of furnaces, if not indeed of human life, if a heat becomes wild and breaks out of bounds. The high cost and risk of such research work as this need scarcely be referred to. However,

in spite of the opposition that always arises in one quarter or another when one proposes to attempt the so-called impossible, the problem was finally solved and the purest rolled iron ever made in the history of metallurgy on the large scale of operation used in steel making entered the commercial field. Some one has truly said of this new metallurgical product that: Ignored in the beginning, assailed in its youth and imitated in its prime, it has awakened and quickened the pulse of the entire iron and steel industry. Ever since the appearance of commercially pure open hearth iron the effort has been made to produce something just as good, or, if possible, something better, and the field that has been exploited in these efforts has been the production of lean alloys of steel or iron with copper or nickel. Numerous papers have been published describing tests made on behalf of these alloys, but the weak point of nearly all of these publications has been that they have started out with the definite purpose of attempting to prove that the desired result had been attained. The paper under discussion draws some sweeping conclusions in the effort to set forth that alloys of American Ingot Iron with small percentages of nickel, cobalt and copper are more resistant to corrosion than American Ingot Iron itself. These conclusions are based on the loss in weight on exposure of small disks about the size of a fifty-cent piece cut from little castings made from a melt in a laboratory crucible. It does not seem to have occurred to the authors that even if their tests are reliable that the test pieces are no longer in any sense of the word representative of American Ingot Iron or its alloys. They are not samples of a commercial rolled metal nor did they receive the same treatment nor work. They are merely little samples manufactured in a laboratory and bear little relation to the original finished metal which served as raw material. The authors of the paper did, however, have the cooperation of the American Rolling Mill Co. to the extent that they were supplied with a series of full commercial sized sheets of cobalt alloys made with an ingot iron base. The authors state in their paper that the tests made on samples cut from these rolled sheets do not bear out the conclusions based on the tests of the laboratory samples.

The American Rolling Mill Co. has spared no expense in putting out tests and has now in its proving grounds more than a thousand full-size commercial sheets and other shapes undergoing various kinds of exposure tests. Speaking for this company, I may say that it is intensely interested in the possible discovery of a practical alloy on

an ingot iron base that will by its excellence justify its manufacture. No such alloy has as yet been developed as the result of systematic and numerous tests running over a number of years, although some of them are not altogether without promise for the future. The results of our tests are not at all in agreement with those set forth in the paper under discussion, which concludes that ingot iron alloys of nickel, cobalt and copper are more resistant to corrosion than American Ingot Iron itself, but it is quite evident that the authors were not testing ingot iron alloys, but samples of laboratory manufacture. The paper itself contains internal evidence of the futility of basing sweeping conclusions on the weight losses of tiny laboratory samples.

The difference in the corrosion factor derived from duplicate samples of the same heat show a wider variation in many cases than is shown between the samples of the various alloys. To select a few instances only, sample H, 207 (*a*), a .50 per cent nickel alloy, showed a corrosion loss factor of 510 while its duplicate, H, 207 (*c*), showed a loss of 905. A .22 per cent cobalt alloy, 34204 (*a*), sample cut from a commercial sheet, showed a corrosion loss of 1180, while its twin, 34204 (*b*), cut from the same sheet, showed a loss of 292, a greater difference than is shown between any of the average differences between the competing types that were under test.

Based on very similar methods of testing, E. A. and L. T. Richardson [9] have concluded that the addition of copper to pure iron has a very slight influence on corrosion resistance, while the present authors conclude rather vaguely that "it seems to be conducive to reducing the corrosing of American Ingot Iron under atmospheric conditions."

As I have stated before, no one has been more concerned than the manufacturers of American Ingot Iron to discover whether the alloys of cobalt, copper and nickel, as well as many other elements, with pure iron were superior to a carefully standardized and guaranteed purity of iron itself. Not only atmospheric corrosion, which is rarely encountered by uncoated sheet iron in service, but all the multiform and variable conditions of service exposure have been studied. Not wishing to make this discussion too long, it may be stated that although premature publication of results has been carefully avoided, the conditions noted in more than a thousand full-size commercial sheets do not agree with the conclusions of the authors,

[9] Paper before American Electrochem. Soc., Sept. 30, 1916.

who seem to have been a little troubled by this tendency themselves. Having arranged cooperation with The American Rolling Mill Company after supplying the cobalt material, they received a number of full-size cobalt alloy sheets made on the big scale of operation usual in open-hearth practice. They state:

" The samples, numbers 34175 and 44009, prepared by The American Rolling Mill Company, so far as this series alone is concerned, do not tend to bear out the conclusions from the series prepared at the laboratory. From these samples alone it would seem that additions of small amounts of cobalt, up to 1 per cent, have very little effect on corrosion, and that the addition of nickel to about 0.7 per cent was harmful."

If the much-debated question of the possible effect of a small copper content on resistance to atmospheric corrosion is ever to be finally settled, the conclusion must be based on unbiased, non-partisan tests made on large test pieces of commercial size. With the full realization of this truth, about two years ago I suggested to Committee A–5 of the American Society for Testing Materials that the Committee take up this investigation in a systematic manner. At great financial expense and at the cost of the time of a number of us who have cooperated in this work, three sets of exposure panels, containing thousands of full-size commercial sheets of varying analysis and types, have been exposed on substantial wooden racks in three separate parts of the country: at the Naval Academy, Annapolis, representing salt-air conditions; at Pittsburgh, Pa., representing manufacturing conditions; and at Fort Sheridan, Ill., representing inland rural conditions. The sheets at Annapolis and Pittsburgh are already under test, and those at Fort Sheridan will soon be completed. Until results have been obtained from these tests and thoroughly digested and reported on by unbiased observers representing consumers as well as producers, it would be well for those who are conservatively inclined to reserve judgment in regard to the effects of small amounts of alloying constituents on corrosion resistance. The danger already exists, as I have so frequently had occasion to point out, of drawing sweeping conclusions as the result of inadequate tests, which are not only incorrect, but in many cases quite the opposite of the truth.

Mr. KALMUS: Dr. Cushman refers to our " cooperation " with the American Rolling Mill Co. We received their products for our tests in exchange for a considerable batch of very pure metallic cobalt.

Beyond that, I recall no cooperation being contemplated for the four series of observations on small samples which were made for the Canadian Bureau of Mines, primarily for the purpose of studying the effect of additions of metallic cobalt. Further independent publications were contemplated regarding the observations on the rolling mill sheets (Series V.), concerning which there was considerable correspondence regarding cooperation. This was in 1913, about four years ago, but nothing has since been done by either party about cooperative research, except that each supplied the other with analyses of materials submitted. This has been acknowledged in the text. I take this opportunity again to thank the American Rolling Mill Company and, in particular, Dr. Beck, Director of their Research Department, for many courtesies in connection with our exchange of materials. The present paper is not intended to cover Series V, i.e., observations on the rolling mill sheets. We state " it will take at least another year for these sheets to corrode through to destruction, before which time no final conclusions can be drawn." It is contemplated that our observations on these sheets should form a complete and independent paper to be published more or less simultaneously with those of the American Rolling Mill Company on the same or similar sheets.

Dr. Cushman says that our samples " bear little relation to the original finished metal which serves as raw material," and that they are not " representative of American Ingot Iron or its alloys."

We make no claim that they are representative of American Ingot Iron, on the contrary, we report that the new alloys formed by addition of small percentages of copper, nickel and cobalt are different from the American Ingot Iron used as a base, in that they were less corroded by the continuous action of the atmosphere during nine months of exposure. We state clearly how these samples were prepared and make no claim that they bear any relation to American Ingot Iron, other than that the latter was used as a base or source of iron, and as a standard of corrosion comparison.

Dr. Cushman remarks, " The authors state in their paper that the tests made on samples cut from these rolled sheets do not bear out the conclusion based on the tests of the laboratory samples." We made no such statement in the paper. In the first place, we cut no samples from the sheets. All samples were cut from bars. The remark probably refers to conclusion four following series two. Dr. Cushman failed to note that this conclusion was from a preliminary

series of measurements on samples which were unannealed. We carefully limit that conclusion by the phrase " so far as this series alone is concerned," namely series two, and because the samples of this series were unannealed, it was regarded as preliminary, and another full series of alloys prepared.

The next series of measurements (series 3), were made on annealed samples from which most of the general conclusions were drawn. This series indicates that the samples prepared by the addition of small percentages of copper, nickel and cobalt (from 0.25% to 3.0%), to American Ingot Iron as described in the text of the paper are more resistant to atmospheric corrosion than the original American Ingot Iron which was used as a base for the preparation of the alloys, and which was used as a standard of comparison.

I take exception to the following remark of Dr. Cushman: " The paper itself contains internal evidence of the futility of basing sweeping conclusions on the weight losses of tiny laboratory samples. The difference in corrosion factor derived from duplicate samples of the same heat show a wider variation in many cases than is shown between the samples of the various alloys. To select a few instances only; samples H 207 (*a*), etc., etc." Dr. Cushman calls attention to a pair of measurements for sample H 207 (*a*) of magnitude 905 and 510, respectively, namely, with an average deviation of individual measurements from their mean of about 28 per cent. For the other sample to which he calls attention he has quoted the figures 1180 and 292, these being the extreme corrosion losses for the sample, and, indeed, for any sample. These figures show an average deviation from their mean of approximately 60 per cent.

Both of the samples to which he makes reference are taken from series one, which is admittedly preliminary. If, in a similar manner, we take the average of the observations for each of the samples in series two, and note the average deviation of a single observation from their means, the resultant average deviation for the entire series two is slightly above 3 per cent. This series represents measurements on nineteen alloys from which none are omitted in the above computation. Again, making a similar computation for series three, which is by far the longest series, and which reports about 125 corrosion measurements, the average deviation of a single observation from their means is about 4 per cent.

Opinions may vary as to whether or not the alloys described in our paper represent what would be produced from American Ingot

Iron by the addition of small percentages of nickel, cobalt and copper following a regular manufacturing procedure. We have drawn no conclusions in this regard, preferring to wait until we are in a position to publish our work on the sheets supplied by the American Rolling Company, which are more representative of such a condition. On the other hand, there can be no doubt but that the corrosion measurements on the discs, described in this paper, on the average show a remarkable concordance among themselves for any given sample.

For example, the very alloy, No. 34204, for which Dr. Cushman quotes from Series 1, the values 1180 and 292 as the corrosion factors, in four independent measurements in Series 3 shows the values of the corrosion factors as follows: 638, 643, 638, 638. The average of these four measurements is 639, with an average deviation of a single observation from the mean of less than 0.2 per cent.

In conclusion let me say that we have no elaborate ideas as to the conclusiveness of our results or of any other single set of measurements on so evasive a subject as atmospheric corrosion. We noted particularly that the primary purpose of our investigation was "measurement and the setting forth of data." These we consider of the first importance, much more so than any conclusions we may have drawn from these data, and we admit that others may properly conclude at variance with ourselves. We do not, however, admit the particular points of exception which Dr. Cushman has raised.

NITRIC ACID SOPHISTICATION, A SERIOUS PRODUCTION MENACE

By JAMES R. WITHROW

Read at the Cleveland Meeting, June 14, 1916

Commercial nitric acid is shown in this paper, to be capable of sophistication by sodium nitrate or other sodium salts—an adulteration which may have a serious effect on production, or upon yields in many chemical operations, and which may easily escape detection when the strength of acid is ascertained by determining specific gravity or in the case of sodium nitrate, by determining NO_3 content as well. It follows that other commercial acids may be subject to similar possibilities.

Dissatisfaction with the nitric acid return from the recovery system in a chemical plant in Ohio led to investigation of various factors entering into the situation. About that time the operating chemical engineer called attention to the fact, which he had just discovered, that the workmen had been frequently noticing a small amount of a white material rattling out of the carboys when they were up-ended to pour out the last of this nitric acid. The discovery was made in a shipment of "Aqua Fortis 40° Pale." A "handful" of the crystals saved for inspection looked suspiciously like poorly developed rhombohedrons of sodium nitrate, though it was felt that they surely must be sodium sulphate or acid sulphate from a "boil-over" during nitric acid manufacture.

A qualitative examination revealed only a trace of sulphates while the nitric test was strong even after much washing. Subsequent partial analysis showed SO_3 1.05 per cent. and Na 37.39 per cent. It seemed probable, therefore, that this particular shipment of acid, at least, was saturated with sodium nitrate. Part of our unduly high nitric consumption and poor recovery could be caused by this fact if sodium nitrate was soluble to any extent in the acid. This seemed improbable when one remembers how easily concentrated acids precipitate soluble salts upon addition to the aqueous

solutions of many such salts. The specific gravity alone had been utilized in checking shipments of acid as they arrived and it is evident that the nitrometer would be also likely to give unreliable results in such a case. Titration would, of course, detect adulteration with any salt but is little used because of the common presence of free sulfuric acid. We at once resorted to evaporation for residual salt determination and proceeded to carefully investigate the whole matter to see if our operation economy difficulties were chargeable to any extent to this sodium nitrate content in the nitric acid. It was obvious that if sodium nitrate were soluble to any extent in nitric acid it would be profitable to get the desired specific gravity or even NO_3 content in nitric acid manufacture by adding sodium nitrate, thus saving the expense of sulphuric acid, yield troubles, up keep and depreciation. At 9 cents per pound at point of shipment, the selling price of the acid in question, the returns on the investment should be reasonable. In fact as an industrial chemical achievement other discoveries and developments in nitric acid production in recent years would be puny in manipulation saving, alongside of this method of "butting up" nitric acid, and also as far as the ledger is concerned.

The solubility of sodium nitrate in nitric acid of various strengths could not be located in the literature for the acid concentration we were using though it does exist for weaker acids (Engel, Compt. Rend. 1887, 911; Solubilities of Inorganic and Organic Substances. A. Seidell, Van Nostrand, 1907, p. 307). This work gives data which shows that 34 per cent HNO_3 (25.5° Bé.) will dissolve 7 per cent, $NaNO_3$ which raises the mixture to about 30° Bé.

It seemed likely that if sophistication had been practiced either solid sodium nitrate was dissolved in weak nitric acid, or in strong nitric acid which was then diluted to 40° Bé., after the whole had been warmed in each case and allowed to cool in contact with excess sodium nitrate to avoid supersaturation; or a saturated solution of sodium nitrate was added to weak nitric acid and the excess (precipitated) sodium nitrate, if any, used in preparing the next batch of sodium nitrate solution; or, strong nitric acid was added to saturated or strong sodium nitrate solution and the mixture diluted to 40° Bé. To such fortified solutions water or sodium nitrate solution could be added to reduce the specific gravity to the required strength if the nitrate addition had raised it. The use of solid

nitrate would require heating of the mixture—a manufacturing difficulty.

Our interest was to right the troubles arising as a result of this bad nitric acid and this was accomplished in part by changing the source of acid. No exhaustive laboratory investigation was conducted but merely sufficient work to insure our position in case of controversy. Though much data was accumulated, its details need not be given, as it was necessarily complex and the end results sufficiently emphasize the facts.

THE POSSIBILITIES OF USING SOLID SODIUM NITRATE

Nitric acid of various strengths was found to dissolve considerable sodium nitrate and this was facilitated by heating the mixture.

Table I shows data obtained when nitric acid (42° Bé.) was diluted with water to various strengths and the specific gravity taken, and these solutions then warmed to slight fumes with solid sodium nitrate and allowed to cool to the indicated temperature in contact with excess nitrate crystals in each case.

TABLE I

HNO_3			Final Solution.		
Sp.gr.	Degrees Baumé.	Per Cent $NaNO_3$ Dissolved on Basis of Acid Taken.	Sp.gr.	Degrees Baumé.	Temperature.
1.381	40	3.24	1.401	41.5	15°
1.381	40	3.96	1.392	40.8	22°
1.343	37	4.70	1.368	39	15°
1.324	35.5	5.74	1.356	38.0	15°
1.037	.....	70.94	1.366	38.9	15°

It follows from the general trend of Curve I, (Fig. 1) which gives the specific gravity of acid which will dissolve the amounts of $NaNO_3$ indicated, that as the specific gravity of the curve falls off, the solubility of sodium nitrate in the nitric acid rapidly increases. Curve II (the upper one) shows the displacement or increase in specific gravity by solution of $NaNO_3$ in the acids of different specific

gravity, showing the great displacement at low acid concentrations.

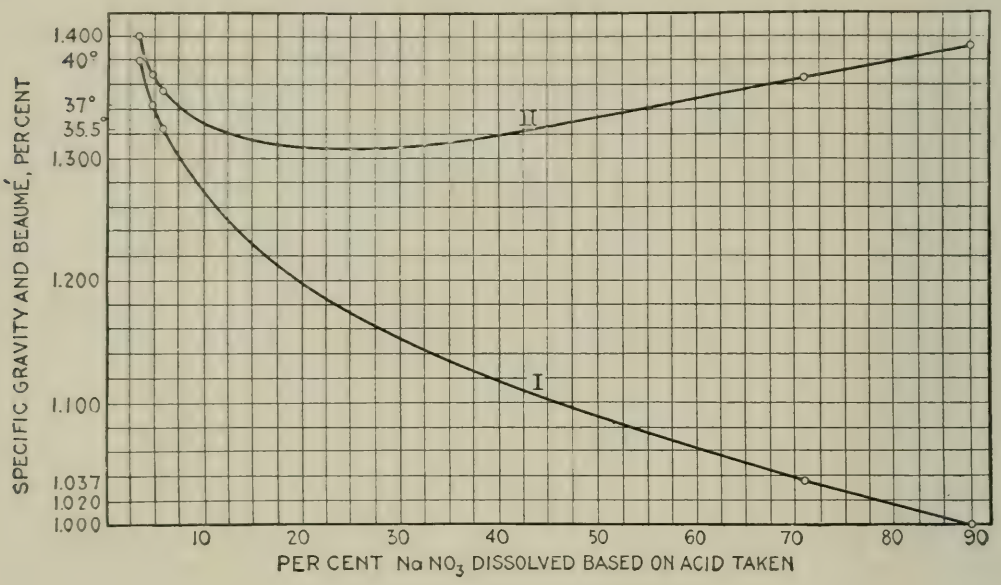

FIG. 1.

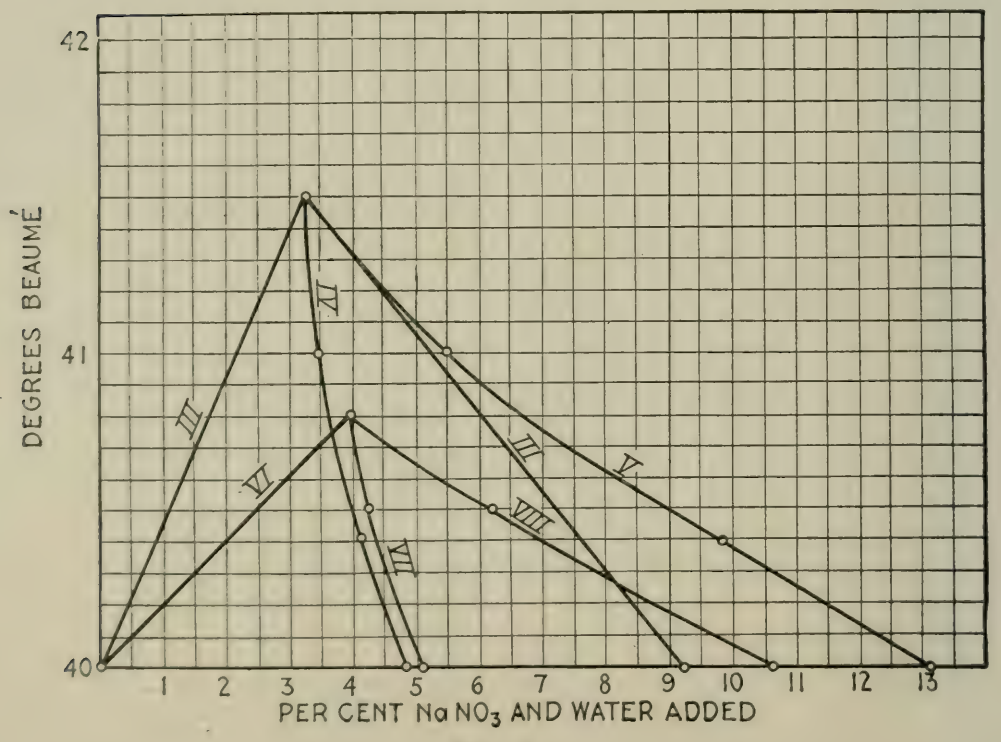

FIG. 2.

To see how much water could be added to a saturated solution of sodium nitrate in nitric acid which was originally 40° Bé. without

lowering the final gravity below 40° Bé., some 42° Bé. acid (sp.gr.=1.408) was diluted to specific gravity 1.381 (40° Bé.) requiring about 9 per cent water. Excess sodium nitrate was then added and in thirty minutes the specific gravity was 1.40 (41.4° Bé.). Water was then added until specific gravity had returned to 1.381 (40° Bé.). The temperature rose slightly during the operation. The nitrate, dissolved at 15° C, was 3.24 per cent of the weight of the 40° Bé. acid used. This permitted 6.0 per cent. water to be added without lowering the gravity below 40° Bé. or 9.24 per cent total addition by weight. Curve III (Fig 2) shows the rise in density and gain in weight due to the solution of $NaNO_3$ in 40° Bé. acid and the fall in density and total gain in weight upon adding water.

When saturated $NaNO_3$ solution containing 46.8 grams $NaNO_3$ per 100 grams, replaced the water in the above work, and the HNO_3 was saturated with 3.24 grams $NaNO_3$ per 100 grams of the original 40° Bé. acid, and all work was conducted at 15° C., not permitting the temperature at any time to go below 15° C., it was found that each addition of nitrate solution caused a separation of solid $NaNO_3$. Eventually $NaNO_3$ had been added to the extent of 4.8 per cent of the 40° Bé. acid taken, and to bring the specific gravity back to 40° Bé. there had been added 8.3 per cent of water or a total addition of 13.1 per cent of the weight of the 40° Bé. HNO_3 used to dissolve $NaNO_3$.

The dilution of the $NaNO_3$-HNO_3 mixture was carried out in steps which may be summarized as follows at the end of each dilution with its accompanying $NaNO_3$ crystals separation:

HNO_3 Stretching with $NaNO_3$ and Water at 15 C.	$NaNO_3$ – HNO_3 Mixture.	Dilution I.	Dilution II.	Dilution III.
Total $NaNO_3$ in solution per 100 grams original 40° Bé. HNO_3	3.24	3.45	4.15	4.83
Water added as $NaNO_3$ solution per 100 grams original 40° Bé. HNO_3	0.00	2.04	5.67	8.30
Total added matter per 100 grams original 40° Bé. HNO_3	3.24	5.49	9.82	13.13
Specific gravity at this dilution	1.399	1.394	1.386	1.380
Degrees Beaumé equivalent	41.4	41.	40.4	40.

Curve IV illustrates the gain in $NaNO_3$ content with falling density of mixture due to dilution with $NaNO_3$ solution and Curve

V shows the rate of total gain in weight by diluting with $NaNO_3$ solution.

A similar series of results were accumulated at higher (ordinary) temperature as more practical working conditions. They may be summarized as follows:

The sodium nitrate solution used for dilution contained 48.33 grams nitrate per 100 grams solution, and its specific gravity was 1.399 at 22.5° C. Separation of $NaNO_3$ crystals took place in each case upon using this solution for diluting the nitric acid-sodium nitrate mixture.

HNO_3 Stretching with $NaNO_3$ and Water at 22.5° C.	$NaNO_3$—HNO_3 Mixture.	Dilution I.	Dilution II.
Total $NaNO_3$ in solution per 100 grams original 40° Bé. HNO_3	3.96	4.26	5.08
Water added as $NaNO_3$ solution per 100 grams original 40° Bé. HNO_3	0.00	1.95	5.53
Total added matter per 100 grams original 40° Bé. HNO_3	3.96	6.21	10.61
Specific gravity at this dilution	1.392	1.388	1.381
Degrees Baumé equivalent	40.8	40.5	40.

Curve VI illustrates the gain in weight and rise in density at 22.5° C. when $NaNO_3$ is dissolved in 40° Bé. HNO_3. Curve VII illustrates rise in $NaNO_3$ concentration with falling density due to dilution with $NaNO_3$ solution, and Curve VIII gives rate of total gain in weight by this dilution.

Although these results amply confirm those in the first table, in this series of experiments the total added matter is lower than was expected. Such reversals of direction, though never serious, and also other interesting solubility phenomena were frequently encountered and the field is an interesting one for further study had one the opportunity. Difficulty in making nitrate solutions that were fully saturated and of avoiding supersaturation even in presence of excess salt was striking at times. No effort was made to insure that solutions were saturated. They were merely made strong and care taken to avoid supersaturation as far as possible. In this particular case, as would be expected, more $NaNO_3$ dissolves in 40° Bé. HNO_3 at 22.5° than at 15°, but the specific gravity is lower in the warmer case. This prevents, therefore, the addition of as much diluent as in the case with less $NaNO_3$ dissolved but with higher density.

Sodium nitrate is *very* soluble in dilute nitric acid. Actual experiment showed that 12.9 weight parts of 42° Bé. acid (sp.gr. 1.408) plus 116.1 weight parts of water had a specific gravity of 1.037 at 15° C. This solution after after saturation by warming with excess sodium nitrate and cooling to 15° C. had a specific gravity of 1.366 (38.9° Bé.) and a $NaNO_3$ content of 70.94 grams per 100 grams of original diluted acid. In other words, 1 part of 42° Bé. acid plus 9 parts of water and 7.1 parts by weight of

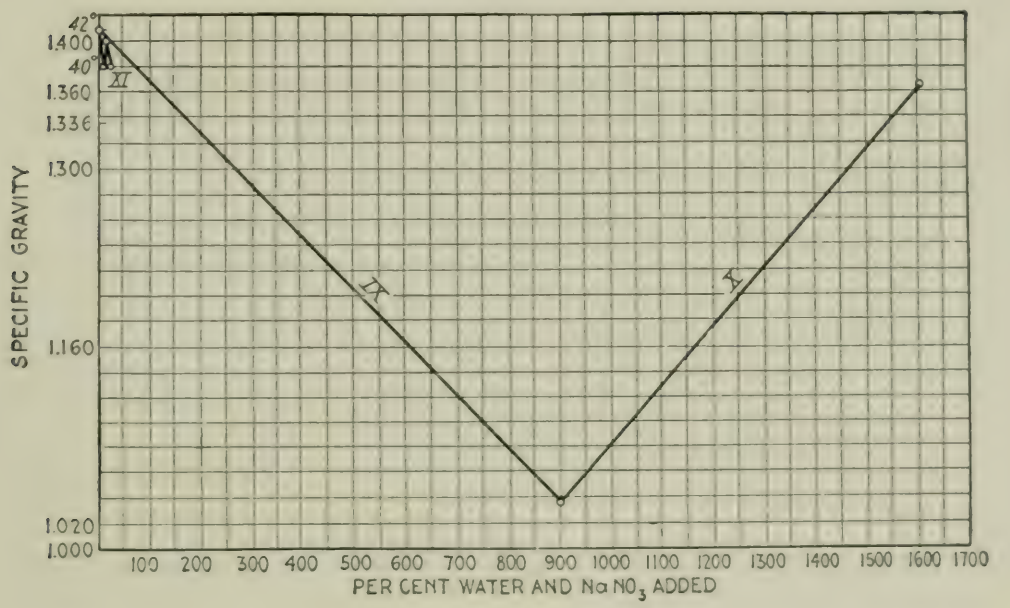

FIG. 3.

$NaNO_3$ give a solution of specific gravity of 1.366 (38.9° Bé.), or 1 lb. of 42° Bé. acid would make 17.1 lbs. of this mixture.

Curve IX (Fig. 3) represents empirically the falling gravity with the addition of 900 per cent of water to 42° Bé. and Curve X in the same way represents the rise in density and gain in weight with the solution of 710 per cent. of $NaNO_3$. Curve XI on behavior of 40° Bé. acid is drawn in for comparison of the possibilities as the acid used becomes more dilute. It had really initiated at 42° and is therefore so shown, otherwise it is identical with Curve III.

POSSIBILITIES OF USING SODIUM NITRATE SOLUTIONS

Since agitating and heating are desirable when dissolving sodium nitrate in nitric acid, and as doing so would require special equipment, such equipment and some difficulties might be eliminated if nitrate were used as a strong or saturated solution, instead of as a solid.

Using C.P. nitric acid a number of solutions were prepared of 10, 30, 40 and 50 per cent strength determined by specific gravity or as high as 35° Bé. A strong solution of sodium nitrate was also prepared at 15°. Its specific gravity was 1.375 and it contained 46.119 g. sodium nitrate per 100 g. solution, or 62.721 g. per 100 cc. of solution. Starting with the same volume of nitric acid, 10 cc. in each case, as the sodium nitrate solution was added, the specific gravity rapidly increased until the quantity of sodium nitrate was somewhat greater than that of the nitric acid, when the specific gravity changed but slightly, approaching, but never reaching, the specific gravity of the original sodium nitrate solution. The highest gravity obtained was about 39.2° Bé., or 1.371 specific gravity, or not quite as high as the solution of sodium nitrate itself 1.375 or 39.6 Bé. This was for the reason that in every case with these strengths of acid, excess sodium nitrate crystallized out upon addition of the nitric acid, or the acid acted merely as a diluent as far as specific gravity of saturated sodium nitrate was concerned.

No analyses of the resulting solutions were made in these cases. Record was kept merely of the initial quantities used and the resulting specific gravities. Without taking up space with the details of the data, the following Curves XII to XV (Fig 4) will give an idea of the tendency of strong $NaNO_3$ solution to boost the specific gravity of the weaker nitric acid under this set of conditions.

These general conclusions were sustained even when stronger acid, 54.4 per cent, sp.gr. 1.343 or 37° Bé. was treated with progressive amounts of a stronger solution of $NaNO_3$ of sp.gr. 1.392 and $NaNO_3$ content of 46.8 grams per 100 grams solution, beginning with smaller amounts of nitrate than previously mentioned. It was found that for each 100 grams of acid 5.15 grams of nitrate solution gave a specific gravity of 1.348. Upon the addition of

10.2 grams nitrate solution separation of sodium nitrate commenced and the highest specific gravity was obtained, 1.352. Progressive additions of nitrate solution were accompanied by continuous elimination of solid sodium nitrate and the specific gravity fell constantly

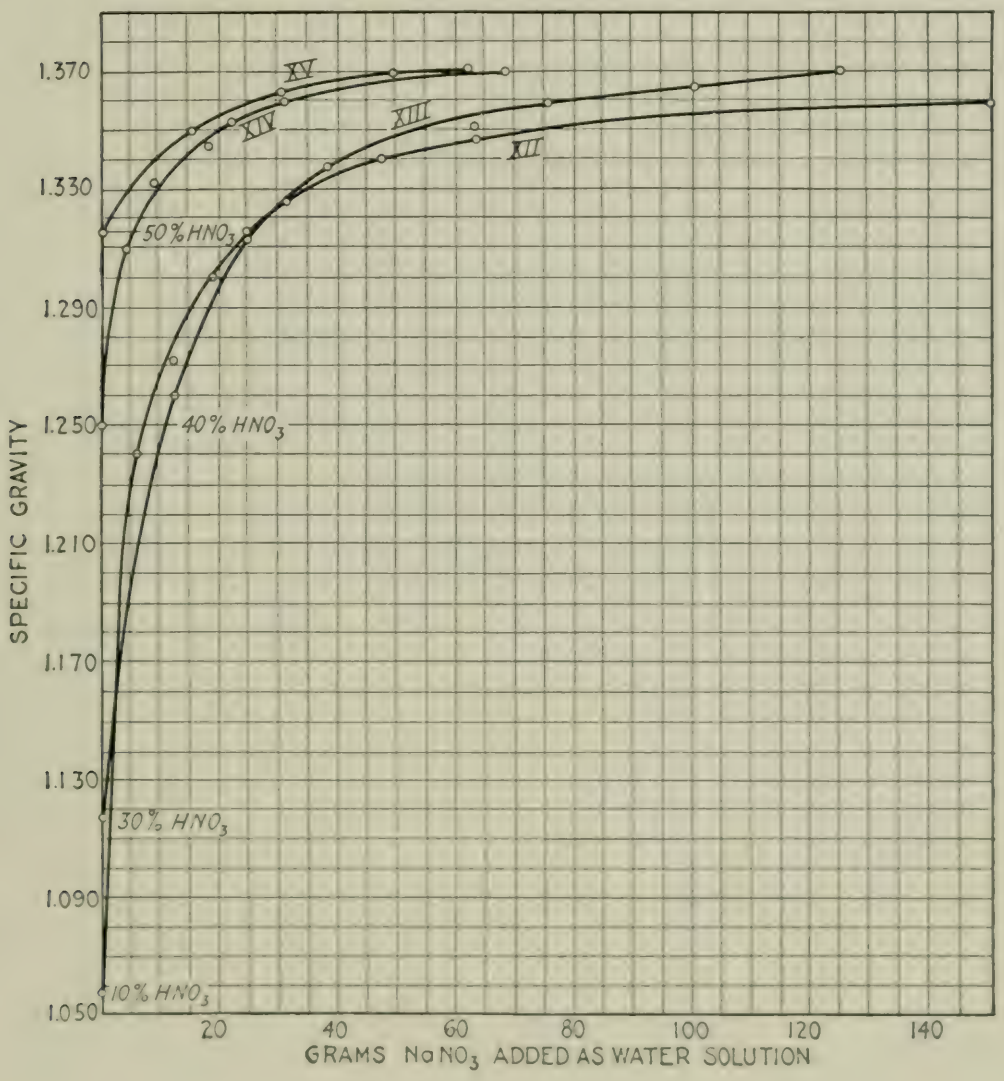

FIG. 4.

as may be seen from the table. No determination of the concentration of nitrate in the resulting acid mixture was made but merely record kept of the progressive change in density after definite addition of nitrate solution.

Specific gravity comparisons of various mixtures of HNO_3 sp.gr. 1.343 and saturated $NaNO_3$ sp.gr. 1.392 follow:

Specific Gravity of Mixture.	$NaNO_3$ Solution Added per 100 Gr. Original HNO_3.	Total $NaNO_3$ Solution Added per 100 Gr. Original HNO_3.
1.343	0.000	0.000
1.344	1.024	1.024
1.348	4.126	5.150
1.352*	5.052	10.202
1.342†	15.263	25.465
1.324	26.60	52.07
1.311	54.63	106.70
1.316	107.53	214.23
1.339	220.57	434.80
1.357	332.12	766.92

* Crystallization began.
† Check on this point was as follows:
Sp.gr. 1.352 +
$NaNO_3$ solution added per 100 grams HNO_3, 10.287.

No crystallization resulted upon addition of a small crystal of $NaNO_3$ and allowing the mixture to stand 12 hours at 15° to 20° C.

Curve XVI (Fig 5) shows graphically the increases and breaks

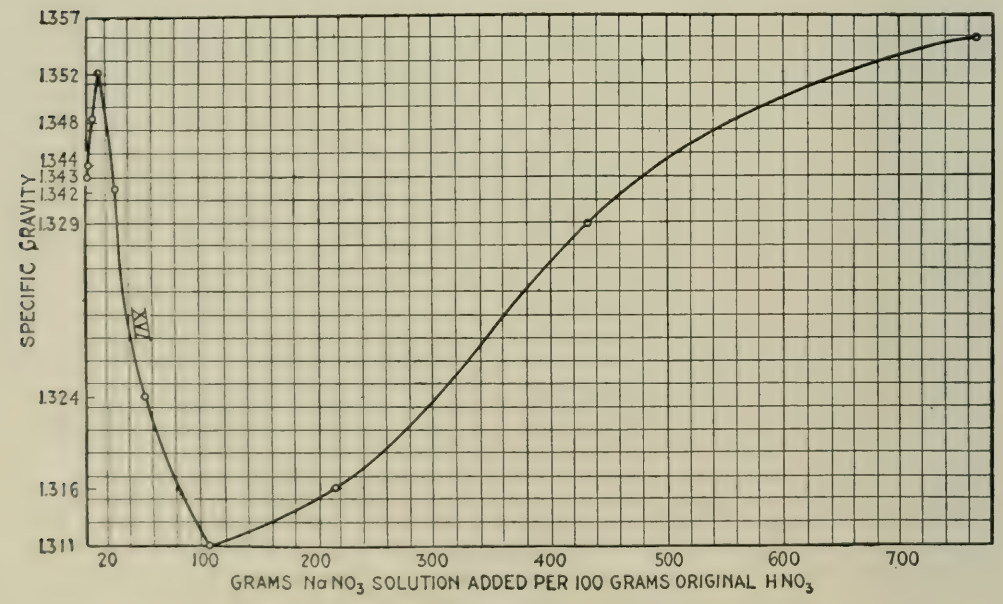

FIG. 5.

in the specific gravity caused by the addition of strong $NaNO_3$ solution to HNO_3 under the above conditions.

Using still stronger acid, to a strong solution of sodium nitrate specific gravity, 1.326 at 15° C. containing 40.8 grams salt per 100 grams of solution, was added 28 parts by weight of 42° Bé. nitric

acid per 131.8 parts of nitrate solution. This solution was allowed to stand 48 hours in contact with a crystal of sodium nitrate. A crop of about 4 grams of good nitrate crystals was obtained, thus preventing supersaturation. The original acid had a specific gravity of 1.408, the sodium nitrate solution a specific gravity of 1.326 and the final mixture stood 1.336 or 36.5° Bé. at 15° C. This final solution contained 32.6 grams of sodium nitrate per 100 grams of the solution plus 17.73 parts by weight of 42° Bé. HNO_3 and 49.67 parts of water exclusive of the water present in the 42° Bé. nitric acid, or 1 part of 42° Bé. HNO_3 per 1.837 parts $NaNO_3$ and 2.79

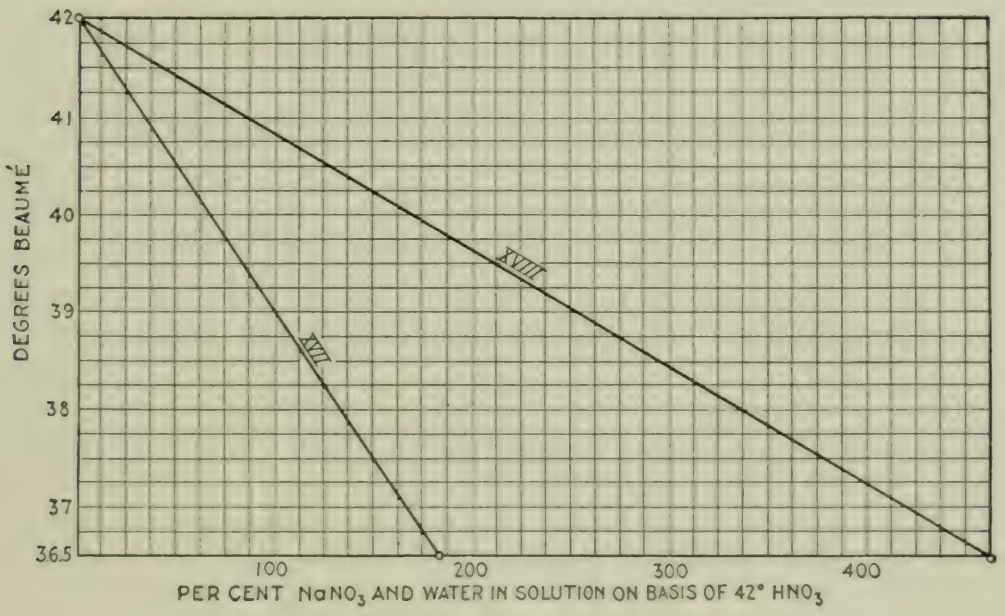

FIG. 6.

parts of water. Curves XVII and XVIII (Fig. 6) show the gain in both $NaNO_3$ and water during dilution with the $NaNO_3$ solution. The 42° Bé. acid used contained 18.8 grams of real HNO_3 and the final product (36.5° Bé.) has a specific gravity corresponding to 83 grams of real HNO_3 or a gain on this basis of over 340 per cent. This apparent gain was produced by adding less than 2.7 lbs. of sodium nitrate per lb. of 42° acid started with.

Using a smaller quantity of strong acid, to 100 grams of a water solution of $NaNO_3$ (sp.gr. 1.393 or 41° Bé.) at 15° was added 2.56 grams HNO_3 sp.gr.=1.408 or 42° Baumé. After standing for 24 hours at room temperature the solution was cooled to 15° C. and decanted from the crystals which had precipitated.

The specific gravity of the solution at this point=1.386 or 40.5° Bé.

$NaNO_3$ content of the saturated water solution of $NaNO_3$ per 100 grams solution=47.430 grams.

This means that our 3900 per cent. of sodium nitrate solution added to 42° Bé. HNO_3 gives a mixture of 40.5° Bé.

SULPHATE AND ACID SULPHATE ADMIXTURE

The separation of nitrate crystals from nitric acid is by no means evidence that nitrate had been mixed with or added to the acid. The solution of sodium sulphate in strong nitric acid was found to deposit small white crystals which are clear-cut rhombohedrons of sodium nitrate. This behavior might be expected from general considerations and in connection with the possibilities of the following equation:

$$Na_2SO_4+HNO_3=NaHSO_4+NaNO_3$$

The original admixture might have been, therefore, sodium sulphate and still have sodium nitrate separate from the acid.

Nitric acid specific gravity 1.381, 40° Bé., was found to dissolve 12.62 grams of sodium sulphate per 100 grams of the acid at 15° C. giving a specific gravity of 1.416 or 42.6° Bé. This was diluted with water in steps to 40° Bé. with results as follows:

HNO_3 Stretching with Na_2SO_4 and Water at 15° C.	Na_2SO_4 HNO_3 Mixture.	Dilution I.	Dilution II.	Dilution III.
Total Na_2SO_4 in solution per 100 grams original 40° Bé. HNO_3	12.62	12.62	12.62	12.62
Total water added per 100 grams original 40° Bé. HNO_3	0.00	2.31	7.91	15.15
Total matter added per 100 grams original 40° Bé. HNO_3	12.62	14.93	20.53	27.77
Specific gravity at this dilution	1.416	1.410	1.392	1.374
Baumé equivalent	42.6	42.2	40.8	39.5

It was also found that the addition of sodium sulphate to 40° Bé. nitric acid could raise it to 45° Bé.

Sodium acid sulphate separates apparently as such, together with some sodium nitrate when added to nitric acid. This acid of 40° Bé. strength was found to dissolve 12.62 grams of sodium acid

sulphate per 100 grams of the acid at 15° C. giving a specific gravity of 1.462 or 45.8° Bé. This was diluted in steps with water to 40° Bé. with the following results:

HNO_3 Stretching with $NaHSO_4$ and Water at 15° C.	$NaHSO_4$ HNO_3 Mixture.	Dilution I.	Dilution II.	Dilution III.
Total $NaHSO_4$ in solution per 100 grams original 40° Bé. HNO_3	12.62	12.62	12.62	12.62
Total water added per 100 grams original 40° Bé. HNO_3	0.00	2.89	16.36	26.68
Total matter added per 100 grams original 40° Bé. HNO_3	12.62	15.51	28.98	39.30
Specific gravity at this dilution	1.462	1.455	1.412	1.382
Baumé equivalent	45.8	45.4	42.3	40.

These results could very easily be obtained using the niter cake from the manufacture of nitric acid. This material is at hand and

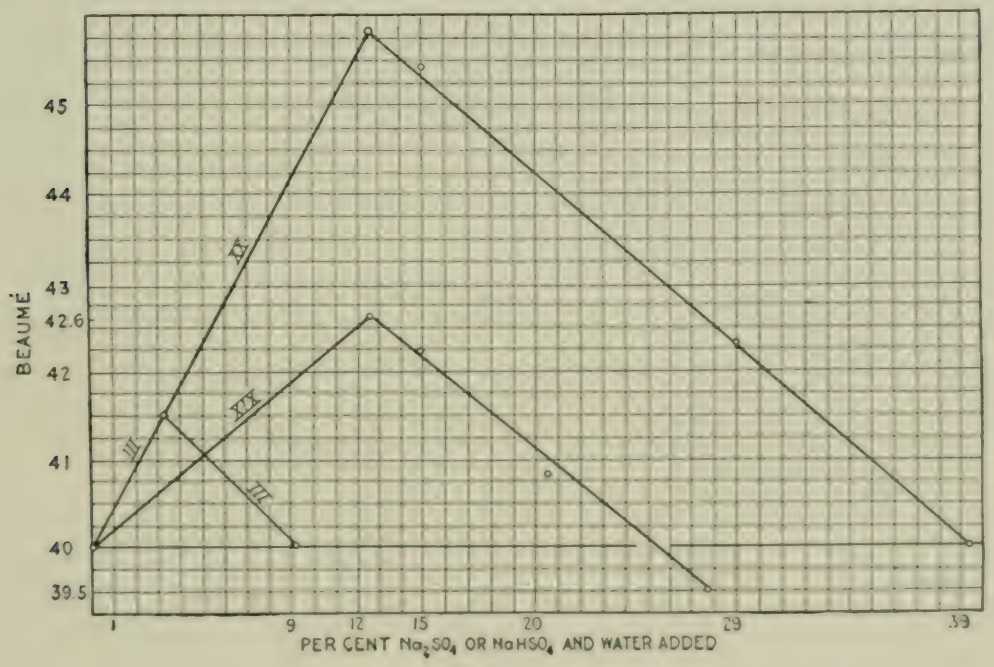

FIG. 7.

convenient and is a drug on the market at present. It is interesting to note that the solubility of sulphate and acid sulphate is the same in this strength acid if equilibrium actually was obtained in both cases. Confirmatory work on this by different persons arrived at the same conclusion. In spite of this, however, the specific

gravity is different in the case of the solutions of these two salts and therefore much more water can be added in the case of the acid sulphate without lowering the specific gravity below 40° Bé.

Curves XIX, Na_2SO_4, and XX, $NaHSO_4$, (Fig. 7) illustrate clearly the real danger to the consumer from the presence of even a little acid sulphate in his nitric acid. Its effect on the density is profound, and when compared with the effect of $NaNO_3$ (Curve III superimposed) is edifying. It was interesting to note that the curve for $NaNO_3$ (III) lay exactly on that for $NaHSO_4$ (XX) as though $NaNO_3$ went into solution as an acid nitrate, for $NaHSO_4$ could hardly go into solution as a nitrate.

FINANCIAL BEARING

Enough results have been given to indicate the possibility of profitable sophistication of nitric acid and at any rate to warn against serious loss to consumers. The indication of a few financial possibilities will suffice to clearly point out the dangers of these opportunities for salt admixture in commercial acid.

Our contract called for "Aqua Fortis 40° Pale" at 9 cents f.o.b. (9.5 cents delivered). The *Oil Paint and Drug Reporter's* quotations, June 19, 1916, were as follows:

Aqua Fortis	36°	7½—7¾
	38°	7¾—8
	40°	8 —8¼
	42°	8¼—8½
Salt Cake (July 3)		70c. per cwt.
Nitrate of Soda		$3.10 per cwt. spot

These are, of course, war prices. The corresponding quotations June 17, 1912 were:

Aqua Fortis	36°	3⅝—4½
	38°	4 —4¼
	40°	4¼—4¾
	42°	4⅝—5
Nitrate of Soda		$2.45 per cwt.
Salt Cake		$0.55-.65 per cwt.

Nitric is quoted from a fraction to a full cent higher for the various strengths.

If cost of materials are figured in the case where 40° Bé. HNO_3 dissolved 3.24 per cent of its weight of $NaNO_3$ and then permitted the addition of 6.0 per cent of water before the specific gravity fell to 40° Bé. again, we reach the following statement:

Buying always at top market and expecting on sale only bottom market,	
100 lbs. 40° Bé. Aqua Fortis at 8¼ cts.	$8.25
3.24 lbs. $NaNO_3$ at 3.1 cts.	.10
6.0 lbs. water	
Total 109.24 lbs. 40° Bé. mixture costing	$8.35
109.24 lbs. 40° Bé. Aqua Fortis at 8¼ cts. brings	9.01
at 8 cts.	8.74
Minimum margin to producer per 100 lbs. initial acid	$0.39
Minimum excess (dead loss) paid by consumer at 8 cts.	0.49
Per 100 lbs. purchased	0.45
Minimum excess at ante-bellum prices per 100 lbs. purchased at 4¼ cts.	0.40

There would have been no excess paid by purchaser under extreme minimum conditions of 4¼ cents in ante-bellum times.

In the case of the data where 40° Bé. acid saturated with $NaNO_3$ permitted the addition of $NaNO_3$ solution to such an extent that a $NaNO_3$ total of 4.8 per cent of the weight of the original 40° Bé. HNO_3 had been added and an addition of 8.3 per cent of water or a total of 13.1 per cent before 40° Bé. was again reached, we have the following situation:

100 lbs. 40° Bé. Aqua Fortis at 8¼ cts.	$8.25
4.8 lbs. $NaNO_3$ at 3.1 cts.	.15
8.3 lbs. water	
113.1 lbs. 40° Bé. mixture costing	$8.40
113.1 lbs. 40° Bé. aqua fortis at 8¼ cts. brings	9.33
113.1 lbs. 40° Bé. aqua fortis at 8 cts. brings	9.05
Minimum margin to producer per 100 lbs. initial acid	0.65
Minimum excess (dead loss) paid by consumer per 100 lbs. purchased at 8 cts.	0.70
Minimum excess at ante-bellum prices per 100 lbs. purchased at 4¼ cts.	0.05
Minimum excess at ante-bellum prices per 100 lbs. purchased at 4¼ cts.	0.54

Cost of materials follows for the case where 40° HNO_3 was diluted with 9 parts of water and then saturated with 7.1 parts of

$NaNO_3$ giving a total of 17.1 parts of a mixture with a specific gravity 1.366 or 38.9° Bé.

100 lbs. 42° Bé. aqua fortis at 8½ cts.	$8.50
710 lbs. $NaNO_3$ at 3.1 cts.	22.01
900 lbs. water.	
Total 1710 lbs. 38.9° Bé. mixture costing	$30.51
1710 lbs. 38° Bé. aqua fortis at 7¾ cts. brings	132.53
Minimum margin to producer per 100 lbs. initial acid	$102.02
Minimum excess (loss) paid by consumer per 100 lbs. purchased at 7¼ cts.	7.28
Minimum excess at ante-bellum prices per 100 lbs. purchased at 4 cts.	3.70

This is a margin over cost of materials of $102.02 on an investment of $30.51, but it will be noticed that the margin is really greater than this for the mixture was 38.9° Bé. and it could stand much more dilution before it reached 38° Bé., the acid quoted at 7¾ cents, and this would materially increase the margin derived above.

The consumer in purchasing 1710 pounds really received but 100 pounds of 42° HNO_3 equivalent to 120 pounds of 38° Bé. acid which should have cost him $9.30 to $9.60 instead of $132.53.

In normal times with prices as quoted, the margin over cost of all materials would be $46 per 100 pounds of 42° Bé. acid invested, and the consumer would pay $68.40 for acid whose cost should not have been above $5.

Cost of materials follows for the case where 42° HNO_3 was mixed with 1.97 parts of $NaNO_3$ and 3.13 of water giving a solution with a specific gravity of 1.336 or 36.5° Bé.

100 lbs. 42° Bé. aqua fortis at 8½ cts	$8.50
197 lbs. $NaNO_3$ at 3.1 cts.	6.11
313 lbs. water.	
Total 610 lbs. 36.5° Bé. mixture costing	$14.61
610 lbs. 36° Bé. aqua fortis at 7½ cts. brings	45.75
Minimum margin to producer per 100 lbs. initial acid	$31.14
Minimum excess (loss) paid by consumer per 100 lbs. purchased at 7½ cts.	6.10
Minimum excess at ante-bellum prices (3⅝ cts.) per 100 lbs. purchased	2.80

Cost of materials follows for the case where 40° HNO_3 was mixed with 12.62 parts Na_2SO_4 and 15.15 parts of water giving a 40° Bé. solution.

100 lbs. 40° Bé. aqua fortis at 8¼ cts	$8.25
12.62 lbs. Na_2SO_4 at 0.7 cts	.09
15.15 lbs. water	
Total 127.77 lbs. 40° Bé. mixture costing	$8.34
127 lbs. 40° Bé. aqua fortis at 8 cts. brings	10.16
Minimum margin to producer per 100 lbs. initial acid	$1.82
Minimum excess (loss) paid by consumer per 100 lbs. purchased at 8 cts.	1.51
Minimum excess at ante-bellum prices per 100 lbs. purchased at 4¼ cts.	.51

In the case where $NaHSO_4$ (niter cake) was used the total matter added was much higher than in the case of sodium sulphate without lowering the specific gravity below 40° Bé. The margin to the producer would be greater therefore than with the normal sulphate and particularly so since niter cake is a drug on the market at present with most acid makers and usually is with many of them.

DISCUSSION

With these quotations in mind it is evident that it would be profitable to buy 42° acid at the top price 8½ or 5 cents in war or normal times respectively and add to it sodium nitrate or a strong nitrate of soda solution or either of the sulphates of soda in such a way as to greatly stretch the amount of acid on hand by diluting with water, and selling at even the lowest quotation for the particular specific gravity obtained.

Not as much salt and water need be present as in the extreme possible cases cited. The profit to the producer and the loss to the consumer can be of any size short of the maximum. It has been shown by the data given that even a casual boil-over of $NaHSO_4$ during distillation may entail an unnecessary loss to the consumer by unduly increasing the producer's yields at the expense of the consumer.

Even 1 cent per pound is a severe raise on the market in the case of 9 cents acid and may spell disaster to many operations.

It seems likely that no fraud was intended in the case which caused this investigation and therefore the name of the manufacturer has been withheld even in private conversation. Further deliveries, however, were refused. It would seem that the presence of the sulphuric radical in the nitrate separating from the acid shows either large amounts of sulphuric acid in the nitric acid or

the results of a boil-over of niter cake or "soup." The fact that the acid always arrived of the correct specific gravity does not signify intentional adulteration, for the final correction of the specific gravity might be made by an employee who had no knowledge that a boil-over had occurred.

The presence of SO_4 in reagent ("pure") HNO_3 has been long known though no commercial information is published. In one case (Chem. News. 61, pp. 289 and 301) the manufacturer suggested that *it came from the "salt cake" used in making the bottles in which the acid was stored!*

It is unlikely that adulteration of commercial acids is practiced to any extent.

Obviously no reputable manufacturer would avail himself of the possibilities indicated in this paper. However, these possibilities must be kept in mind by consumers for even operating employees might at times fall to the temptation to better their yield record in this way.

The possibilities herein disclosed of varying amounts of contamination in commercial nitric acid are not of direct financial significance merely, though that phase is serious enough, for it is evident that the presence of salts in quantity may seriously impede processes as well as curtail yields of same. This last effect may be quite serious where the nitric acid failure even to the extent of a few per cent in one operation may cause a greatly magnified fall-off in the finished product, leaving all question of quality out of consideration.

It should be noticed by consumers of most commercial acids, such as hydrochloric, sulphuric, acetic, that similar possibilities exist in these cases. The solubility of salts in some of these is even greater than in the case of nitrates and with some of them little or no inspection is exercised other than specific gravity. Obviously the determination of the acid radical in most cases would give little protection and titration is not satisfactory with some of them. An evaporation test for residual salt would be the only certain single test to eliminate the factor under discussion.

The author wishes to thank Messrs. F. R. Porter, W. J. Becker, H. Mersereau, S. L. Shenefield and T. A. Boyd for checking data by laboratory experiment at different times.

CONCLUSIONS

I. The strength of commercial nitric acid cannot be derived with safety from the specific gravity alone nor from the specific gravity and NO_3 content alone, but titration will be necessary in addition provided always that sulphuric acid is found to be absent by qualitative tests. It seems desirable to combine specific gravity or NO_3 determination with an evaporation test for residual salts.

II. Producers and consumers alike should inspect all commercial acids by an evaporation test.

Laboratory of Industrial Chemistry,
The Ohio State University.

DISCUSSION

CHAIRMAN: Is there any discussion of Prof. Withrow's paper?

Mr. LIHME: May I ask this: Where you got that nitric acid? Because I don't think there is any responsible manufacturer in this country who would do it purposely, put nitrate of soda into nitric acid.

Prof. WITHROW: I believe that is correct, and as I have not conclusive proof of intention to adulterate I have not mentioned the manufacturer. We discovered it in the last carload.

Mr. LIHME: And it had nitrate of soda in it?

Prof. WITHROW: Yes, sir.

Mr. LIHME: How many per cent.?

Prof. WITHROW: About 3 3-10 per cent.

SECRETARY: Did you buy the acid in small amounts or large amounts?

Prof. WITHROW: I stopped buying from that firm at once. We buy in carload lots.

I don't think that we did them any harm if they were innocent, as there is a good demand for nitric acid. I don't have any proof that they put it in there intentionally. They are not from Ohio, however, Mr. Lihme.

Mr. LIHME: I guess they are not from Ohio. I can say that if it was our company I would be very glad to know it because it was not intentional on the part of the company; they don't want to cheat people; they get money enough anyway.

Prof. WITHROW: There is no question about that, and I have no

evidence that there was any intentional adulteration in this case at all.

Mr. LIHME: Your nitrate of soda in nitric acid is very insoluble.

Prof. WITHROW: Very little, but as the acid becomes weaker the solubility increases. I was surprised to find that at the point we worked it is about 3.3 per cent.

Mr. LIHME: I am surprised that with the present prices this could be so. It could not be a saving because they would have to buy sodium nitrate anyway.

Prof. WITHROW: I know, but they would save sulphuric acid and the trouble of distillation.

Mr. LIHME: That may be, but I cannot understand how anybody can do it. I am glad to know about this; we will investigate to find if we had any accidentally happen.

Mr. SCHUELER: I would like in this connection to call attention to the fact that at the present we are having the same happen with muriatic acid with the calcium chloride in raising the specific gravity. We found some of it in commercial shipments, and perhaps it got in there unintentionally. (Laughter). But, nevertheless, we are paying money for something we are not getting.

Mr. LIHME: You are finding calcium chloride in your muriatic acid? That must be put in intentionally because I cannot see how it would get in any other way; it couldn't; that is done intentionally.

Mr. SCHUELER: The fact remains that we are getting it in the acid right along, and it makes a difference in the action of the acid.

Mr. LIHME: How many per cent are you finding?

Mr. SCHUELER: Well, the most we found so far has been around —I cannot say exactly—but has been around one per cent.

Mr. LIHME: That would not come in accidentally; that is done intentionally.

Mr. SCHUELER: By the way, we have taken it up with the manufacturers, but we haven't heard anything from them.

Prof. WITHROW: Maybe that calcium chloride got in there in the way that happened down in Philadelphia one time. There was a chemical manufacturer in Philadelphia a number of years ago, who was making chemicals for the United States Mint, and had a profitable contract; in fact, he was more of a politician than anything else. He had some hydrochloric acid towers. I remember the last tower very well, and he was down at Atlantic City one

time and saw a lot of clam shells on the beach. He had been thinking a lot of absorption problems in his towers and thought the shells would be just the thing. So he had them gathered on the beach and brought up to Philadelphia and packed his towers with those clam shells to absorb his hydrochloric acid, with the inevitable result that was to be expected.

A NEW METHOD FOR THE CONTROL OF THE CHAMBER PROCESS FOR MAKING SULPHURIC ACID

By ANDREW M. FAIRLIE

Read at the New York Meeting, Jan. 12, 1917

To "control" the chamber process for making sulphuric acid is to so adjust the proportions of sulphur dioxide and of oxides of nitrogen in the gas mixture admitted to the front chamber as to yield constantly the largest percentage of recovery of the oxides of nitrogen in the Gay-Lussac tower. In order to secure the largest possible percentage of niter recovery a definite composition of the gas mixture entering the Gay-Lussac tower is desirable, and it may not be amiss, at the outset, to refresh the memory as to what this desirable composition is.

The recovery of the nitrogen oxides in the Gay-Lussac tower is most nearly complete when, in the gas mixture entering that tower, the proportions of nitric oxide (NO) and of nitrogen peroxide (NO_2) are such that the combination gives nitrogen trioxide (N_2O_3), with no excess of either of the constituent gases. This desirable combination of the two oxides of nitrogen first mentioned cannot be maintained—cannot even be secured—without the presence in the gas mixture of a small quantity of sulphur dioxide to keep a sufficient amount of the nitrogen gases reduced to the nitric oxide form. If the quantity of sulphur dioxide present be deficient, or if there be none at all, the uncombined oxygen, of which there is normally present in the gas mixture entering the Gay-Lussac, at least, 4 per cent, will speedily oxidize a part of the nitric oxide, thus producing an excess of the nitrogen peroxide, above what is needed to combine with the nitric oxide to form nitrogen trioxide, and this excess, being but slightly soluble in the cold sixty-degree sulphuric acid with which the tower is fed, will be lost, and the fact will be attested by the escape of red fumes from the exit stack of the tower. If, on the other hand, the amount of sulphur dioxide in the gas mixture entering the Gay-Lussac be allowed to go above the normal

percentage, an excess of nitric oxide, above the quantity needed to combine with nitrogen peroxide to form the trioxide, will be present. As nitric oxide likewise is only slightly soluble in cold sulphuric acid of sixty-degree strength, this excess will be largely lost, as in the case of the former assumption; only, because the nitric oxide is a colorless gas, the loss of niter will not be as readily detected by observation of the color of the gases escaping from the top of the tower, as when there is an escape of the red gas, nitrogen peroxide.

Most chamber operators, using the methods of chamber control which have been considered, for many years, standard methods, unconsciously strive to maintain in the gas mixture entering the Gay-Lussac the desirable combination of oxides of nitrogen just described. These standard methods of chamber control are: (1) To observe the differences between chamber temperatures at different parts of the system, learning by experience what differences yield the best recovery of niter in the Gay-Lussac tower, and endeavoring thereafter to maintain such desirable differences constant. (2) To observe the color of the gases in the chambers or connecting flues by means of windows or "sights." (3) To use both of the above methods in combination.

These methods may, perhaps, be considered by some as satisfactory when used at plants where the gas entering the Glover tower always contains a substantially uniform percentage of sulphur dioxide; but it is questionable if, even under the best of conditions as regards uniformity of grade of burner gas, these methods are adequate to yield the most economical results. Certainly when the gas entering the Glover tower is fluctuating in character, as is the case with plants manufacturing acid as a by-product of zinc or copper smelting operations, the old, established methods of chamber control fail, and the need of a new, more reliable method is distinctly felt.

The temperature-difference method fails, when operating on a fluctuating burner gas, because this method is liable to lead the chamber operator to erroneous conclusions, causing him to apply improper remedies. The same change in the temperature differences may be due to one or more of several different causes, such as changes in the composition of the burner gas, changes in the atmospheric temperature, rain, or excessive or deficient supply of nitrogen compounds in the gas mixture within the chambers. On this account the observance of temperature changes alone is an unsafe guide in

regulating the chamber process, and leads at times to serious losses. This method of controlling the chambers is plainly an inexact method.

The color method alone is altogether inadequate for regulating any chamber process. It is customary among those using this method of control to view the color of the gases in the last chamber, or, at best, in an intermediate chamber, where variations in shade are more easily perceptible than in the front chamber. Such information as is gained from noting the color changes in an intermediate or a back chamber comes too late, however, to be of much practical use to the observer. For, if anything is wrong with the proportions of the various gases within the chambers, the fact is not known until the chambers are full of the troublesome gas mixture at least up to the point where the color of the gases is observed, and it is then impossible to apply the remedy in time to prevent losses in the Gay-Lussac tower. Even if the sight glasses be placed in the front chamber, the color there is so obscured by the white mists coming from the Glover tower and still uncondensed, that few persons possess the ability to detect the differences in shade, by day and by night, which would lead to correct inferences regarding the composition of the chamber gas. Furthermore, to control the chambers by observing the color of the chamber gases is a method so inexact that it may be relegated to the "rule-of-thumb" class as a method thoroughly unscientific.

The use of both the temperature-difference method and the color method, in combination, is perhaps a better plan than to use either one alone, as one sometimes helps where the other fails. But, at best, this means the use of two inexact methods, neither of which can be absolutely depended upon to lead to correct deductions in time to avoid losses which might be saved. The method here described,[1] is a method which appeals to practical men as well as scientific men, and is nothing more nor less than control by means of analysis of the gases.

THE ANALYTICAL METHOD OF CONTROL

The analytical method of control for the chamber process consists in determining the percentage of sulphur dioxide in the chamber gases at some point near the front end of the chamber system, and in comparing such percentage with the percentage of sulphur dioxide in the burner gas, before admixture with niter fumes. At any given

[1] See U. S. Patent, 1,205,723.

point in the chambers or chamber gas connections near the front end of the system, the percentage of sulphur dioxide bears a definite ratio to the percentage of sulphur dioxide in the burner or furnace gas, which is free from nitrogen compounds, for any one grade of burner gas, *provided* the oxides of nitrogen and the sulphur dioxide in the chambers are maintained in the proportion required to produce the most nearly complete recovery of nitrogen oxides in the Gay-Lussac tower. Any excess of nitrogen oxides will be definitely indicated by a decrease in the sulphur dioxide ratio below what is desirable; and conversely any deficiency of oxides of nitrogen will be definitely indicated by an increase in the sulphur dioxide ratio, above what is desirable, such decrease or increase in the ratio being an accurate measure of the extent of the excess or the deficiency of nitrogen oxides.

To establish this method of control at any plant, it is necessary to select a suitable point in the chamber system, to determine once for all by repeated sulphur dioxide determinations, what is the desirable ratio between the sulphur dioxide percentage at the point selected, and the percentage of sulphur dioxide in the burner or furnace gas, for each of the various possible grades of burner gas (meaning by "grade of gas" its percentage of sulphur dioxide), and thereafter to so adjust the quantities of nitrogen oxides and sulphur dioxide admitted to the chambers as to maintain the desirable sulphur dioxide ratio corresponding to the particular grade of burner gas in use.

A necessary preliminary to the development of the analytical method of control of the chamber process was the discovery of a method at once rapid and accurate, for the determination of sulphur dioxide in chamber gas. The most rapid and convenient method for the determination of sulphur dioxide in burner gas was that known as Reich's method.[2] This method, while sufficiently accurate for burner gas, is found, when applied to a gas mixture containing oxides of nitrogen, to be decidedly inexact. The nitrogen oxides present in the gas mixture tend to decompose the hydriodic acid as it is formed in the absorption bottle, liberating free iodine. "It is obvious that if the known quantity of iodine which is introduced into the absorption bottle for the purpose of making an accurate assay be increased by an unknown quantity liberated by the oxides of

[2] Geo. Lunge: Manufacture of Sulphuric Acid and Alkali; 4th ed. Vol. I, p. 571 ff.

nitrogen from the hydriodic acid formed in the early stages of an assay or in a previous assay, the results are vitiated. This liberation of free iodine from the products of a preceding assay, or of the early stages of an assay, is attested by the prompt recurrence of the starch-iodine blue color immediately after that color has been discharged, when operating on gases containing oxides of nitrogen. The inference is that this liberation of free iodine is progressing all the time that the gas is being aspirated; hence that, at least, more iodine is acted upon by the gas than the measured quantity introduced for the assay; hence that more gas is drawn through the absorption bottle than would have been necessary to decolorize exactly the amount of iodine introduced for the assay; hence, that the indicated result, in percentage of sulphur dioxide, is too low. Furthermore, duplicate tests made on the same gas in rapid succession will not, if oxides of nitrogen be present, produce corroborative results, thus indicating that the action of the oxides of nitrogen in liberating free iodine is uncertain and irregular, and cannot be correctly allowed for in calculating results.

" In the front and intermediate chambers, where the percentage of sulphur dioxide is relatively high as compared with that of the last chamber, it is usually possible, in attempting to make an estimation of sulphur dioxide by the ordinary Reich test, sooner or later to discharge the blue color of the solution; but the indicated results will invariably be too low, and corroborative results cannot be obtained. The ' end point ' is uncertain, and often the operator, when under the impression, from the increasing paleness of the solution, that the color is about to be completely discharged, finds that the blue color suddenly becomes more intense, instead of continuing to fade. In the last chamber, where the percentage of sulphur dioxide approaches the minimum, it is often entirely impossible to decolorize the solution, due to the larger ratio of the higher oxides of nitrogen to sulphur dioxide therein.

" It is therefore plain that the ordinary Reich test fails to give accurate and reliable indication of the SO_2 content when applied to gases containing the oxides of nitrogen present in chamber gas, and in the last chamber of a sulphuric acid plant often fails to give any indication whatever. However, the Reich test, as applied to gases free from oxides of nitrogen, is so convenient, rapid, and accurate, that it seemed the most desirable method possible for testing chamber gas, if only some means could be found to prevent the interfering

action of the oxides of nitrogen, without impairing the reliability of the test." [3]

Raschig [4] has pointed out the inaccuracy of Reich's test as applied to gas mixtures containing nitrous gases, owing to the recurrence of the blue color above mentioned. To obviate this difficulty, he recommended the use of a solution of sodium acetate, and the filtration of the chamber gases through glass wool before reaching the absorption bottle. The use of sodium acetate delays to some extent the recurrence of the blue color, but experience indicates that results with the Reich test on chamber gas, and especially on chamber exit gas, are still inaccurate and uncertain, when working with the addition of sodium acetate solution alone. It is only when using in the absorption bottle a solution of sodium acetate in combination with acetic acid added in considerable quantity, that the Reich test yields with all kinds of chamber gas or chamber exit gas results that inspire confidence in the accuracy and reliability of the test. Moreover, when using acetic acid in combination with sodium acetate, the filtration of the chamber gases through glass wool—a procedure which is inconvenient where many tests must be made rapidly—may be omitted.

The method of analysis [5] which has been found to be of the greatest value in connection with the development of and actual practice with the analytical method of control for the chamber process, is as follows:

Prepare a cold saturated solution of acetate of sodium, which has always a few undissolved crystals of the salt at the bottom of the container, evidence that the solution is saturated. Measure the volume of the solution prepared, and add to it one-fifth that volume of 95 per cent acetic acid. Of this mixture, called "sodium acetate mixture," add 10 c.c. to the absorption bottle of a Reich apparatus, which contains also (when operating on front chamber gas), 1 c.c. of N/10 iodine solution, five or six drops of starch solution, and about 100 c.c. of water. Proceed then with the test as usual with the Reich test, when operating on burner gas, allowing the water used in aspirating the gas to fall into a 100-c.c. measuring cylinder. When operating on back chamber gas, use instead of N/10 iodine, 1 c.c. of N/50 iodine solution. The tables hereto appended interpret the quantity of water used in aspirating into percentages of sulphur dioxide in chamber gas, for 1 c.c. of N/10 iodine and 1 c.c. of N/50 iodine, respectively.

[3] U. S. Patent, 1,205,723; p. 4. [4] Z. angew. Chem., 1909, p. 1182.
[5] U. S. Patent, 1,205,724.

TABLE A

PER CENT SO_2 IN CHAMBER GAS, USING 1 C.C. N/10 IODINE

C.C. Water.	Per Cent SO_2.	C.C. Water.	Per Cent SO_2.	C.C. Water.	Per Cent SO_2.
36	3.00	74	1.48	165	.67
37	2.92	76	1.44	170	.65
38	2.85	78	1.41	175	.63
39	2.78	80	1.37	180	.62
40	2.71	82	1.34	185	.60
41	2.65	84	1.31	190	.59
42	2.58	86	1.28	195	.57
43	2.53	88	1.25	200	.55
44	2.47	90	1.22	210	.52
45	2.42	92	1.20	220	.50
46	2.36	94	1.17	230	.48
47	2.32	96	1.15	240	.46
48	2.27	98	1.12	250	.44
49	2.22	100	1.10	275	.40
50	2.18	105	1.05	300	.37
52	2.10	110	1.00	325	.34
54	2.02	115	.96	350	.32
56	1.95	120	.92	375	.30
58	1.88	125	.89	400	.28
60	1.82	130	.86	450	.25
62	1.77	135	.82	500	.22
64	1.71	140	.79	600	.19
66	1.66	145	.76	700	.16
68	1.61	150	.74	800	.14
70	1.57	155	.71	900	.12
72	1.52	160	.69	1000	.11

TABLE B

PER CENT SO_2 IN CHAMBER GAS, USING 1 C.C. N/50 IODINE

C.C. Water.	Per Cent SO_2.	C.C. Water.	Per Cent SO_2.	C.C. Water.	Per Cent SO_2.
36	.62	74	.30	130	.17
38	.58	76	.29	135	.17
40	.55	78	.29	140	.16
42	.52	80	.28	145	.15
44	.50	82	.27	150	.15
46	.48	84	.27	155	.14
48	.46	86	.26	160	.14
50	.45	88	.25	170	.13
52	.43	90	.25	180	.12
54	.41	92	.24	190	.12
56	.40	94	.24	200	.11
58	.38	96	.23	220	.10
60	.37	98	.23	240	.09
62	.36	100	.22	260	.09
64	.35	105	.21	280	.08
66	.34	110	.20	300	.07
68	.33	115	.19	350	.06
70	.32	120	.19	400	.06
72	.31	125	.18	500	.04

The following table gives the percentages of sulphur dioxide in burner gas corresponding to the amount of water aspirated, when using 10 c.c. of N/10 iodine solution.

TABLE C

PER CENT SO_2 IN BURNER GAS USING 10 C.C. N/10 IODINE

C.C.	Per Cent.	C.C.	Per Cent.	C.C.	Per Cent.	C.C.	Per Cent.	C.C.	Per Cent.
1103	1.0	360	3.0	212	5.0	148	7.0	113	9.0
1002	1.1	348	3.1	207	5.1	146	7.1	111	9.1
917	1.2	337	3.2	203	5.2	144	7.2	110	9.2
846	1.3	327	3.3	199	5.3	142	7.3	109	9.3
785	1.4	317	3.4	195	5.4	139	7.4	108	9.4
732	1.5	307	3.5	191	5.5	137	7.5	106	9.5
685	1.6	298	3.6	188	5.6	135	7.6	105	9.6
644	1.7	290	3.7	184	5.7	134	7.7	104	9.7
608	1.8	282	3.8	181	5.8	132	7.8	103	9.8
575	1.9	275	3.9	178	5.9	130	7.9	102	9.9
546	2.0	267	4.0	175	6.0	127	8.0	100	10.0
519	2.1	261	4.1	172	6.1	126	8.1	99	10.1
495	2.2	254	4.2	169	6.2	125	8.2	98	10.2
473	2.3	248	4.3	166	6.3	123	8.3	97	10.3
453	2.4	242	4.4	163	6.4	122	8.4	96	10.4
435	2.5	230	4.5	160	6.5	120	8.5	95	10.5
417	2.6	231	4.6	158	6.6	118	8.6	94	10.6
402	2.7	226	4.7	155	6.7	117	8.7	93	10.7
387	2.8	221	4.8	153	6.8	115	8.8	92	10.8
373	2.9	216	4.9	150	6.9	114	8.9	91	10.9

By means of the above-described improvement in the Reich test for sulphur dioxide, with appended tables, it is possible to establish the desirable ratios between the sulphur dioxide percentages of the burner gas and the front chamber gas. Table D and Fig. 1 illustrate the manner in which established ratios may be tabulated and plotted for the guidance of the chamber operator.

In practice the chamber operator ascertains the percentage of sulphur dioxide in the gas entering the Glover tower. He then tests the front chamber gas and ascertains the percentage of sulphur dioxide there. Next he refers to the table of established ratios and finds thereon the percentage of sulphur dioxide for the front chamber corresponding to the given sulphur dioxide percentage in the burner gas. If the front chamber test is lower than the figure indicated as desirable by the table, that is an indication of too much niter in the chamber gas mixture, and the quantity admitted at the front end

of the plant must be diminished at once. If, on the other hand, the chamber test shows a higher percentage of sulphur dioxide than is desirable according to the table, then the quantity of niter must be increased, or the quantity of sulphur dioxide admitted to the chambers must be diminished.

In plants where the process is regular, and the percentage of sulphur dioxide in the gas entering the Glover is substantially uniform throughout the day, it is probable that a few tests each shift would serve to regulate the quantity of niter introduced. But where the

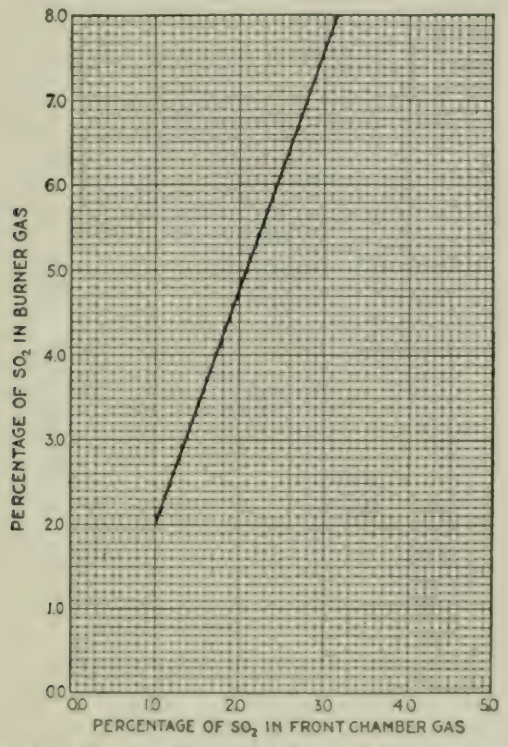

FIG. 1.—Curve showing for various grades of burner gas, the desirable SO_2 percentages in front chamber gas, as determined by experiment.

gas is of a variable or fluctuating character, more frequent tests may be needed, the actual frequency depending on the extent of the fluctuations in the burner or furnace gas. In extreme cases, and in plants where the scale of operations is large enough to warrant the expense, it may be necessary to have one man detailed to do nothing but test the furnace gas, and another to test the chamber gas, compare with the furnace gas, and thereby regulate the process. In such cases the tests may be made very frequently, even as often as every ten minutes, but even in the worst cases it is not worth while to test more frequently than that.

TABLE D

Burner Gas. Per Cent SO_2.	Chamber Gas. Ch. No. 1.		Burner Gas. Per Cent SO_2.	Chamber Gas. Ch. No. 1.	
	C.C. Water.	Per Cent SO_2.		C.C. Water.	Per Cent SO_2.
7.6	36	3.00	4.7	55	1.98
7.5	36	3.00	4.6	56	1.95
7.4	37	2.92	4.5	57	1.91
7.3	37	2.92	4.4	58	1.88
7.2	38	2.85	4.3	60	1.82
7.1	38	2.85	4.2	61	1.80
7.0	39	2.78	4.1	62	1.77
6.9	39	2.78	4.0	64	1.71
6.8	40	2.71	3.9	65	1.69
6.7	40	2.71	3.8	66	1.66
6.6	41	2.65	3.7	68	1.61
6.5	41	2.65	3.6	70	1.57
6.4	42	2.58	3.5	71	1.55
6.3	42	2.58	3.4	72	1.52
6.2	43	2.53	3.3	74	1.48
6.1	44	2.47	3.2	76	1.44
6.0	44	2.47	3.1	78	1.41
5.9	45	2.42	3.0	80	1.37
5.8	46	2.36	2.9	82	1.34
5.7	46	2.36	2.8	84	1.31
5.6	47	2.32	2.7	88	1.25
5.5	48	2.27	2.6	90	1.22
5.4	49	2.22	2.5	94	1.17
5.3	50	2.18	2.4	96	1.15
5.2	51	2.14	2.3	100	1.10
5.1	52	2.10	2.2	102	1.07
5.0	53	2.06	2.1	105	1.05
4.9	53	2.06	2.0	110	1.00
4.8	54	2.02			

It was stated above that, to establish this method of control at a plant, it is necessary to determine the desirable sulphur dioxide ratio for every possible grade of burner gas. From this one would naturally infer that the percentage of sulphur dioxide desirable in the front chamber is not a fixed or constant figure, but on the contrary, is variable, fluctuating in a general way with the fluctuations in the burner gas. Such inference is correct, and the foregoing diagram of established ratios illustrates to what extent the desirable percentage of sulphur dioxide in the front chamber fluctuates. In traversing the chamber system, however, from front end to back, the gas eventually reaches a point where the desirable percentage of sulphur dioxide is practically constant, regardless of ordinary fluc-

tuations in the burner gas. This point will be in the latter half of the chamber system, and at such a distance from the Gay-Lussac tower that the gas would reach that tower normally in about twenty minutes. At this point, the desirable percentage of sulphur dioxide has been found by experience to be about 0.65 per cent, and from this point on, the desirable percentage at any chosen point will be a substantially fixed figure. The desirable percentage will gradually diminish, as the gas approaches the Gay-Lussac tower, until, when that tower is reached, the desirable percentage of sulphur dioxide in the gas mixture reaches the minimum, that is to say, just enough to keep the nitric oxide and the nitrogen peroxide in the right proportions.

The fact that there is in the latter half of the chamber system a region where the desirable percentage of sulphur dioxide is, relatively speaking, a fixed quantity, independent of fluctuations in the burner gas, may be made use of in establishing ratios between the sulphur dioxide percentages in burner gas and in front chamber gas. That ratio which repeatedly yields at the testing place in the latter half of the chamber system the desirable sulphur dioxide percentage will prove to be the desirable ratio to maintain. With a complete set of ratios once established, tests made at the testing place in the latter half of the system will verify the accuracy of the tests made at the front, or will disclose inaccuracies and lack of skill in adjusting the quantity of niter needed to meet the requirements. In practice, the chamber operator himself frequently makes this secondary test in the latter half of the system as a check on his work at the front. Inasmuch as this check is of the greater value, the sooner it is obtained, obviously the best place for the secondary chamber test is at the front part of that region where the desirable sulphur dioxide percentage is stable; that is to say, at the point where the sulphur dioxide percentage is normally 0.60 to 0.70 per cent.

The precise percentage of sulphur dioxide present in the gas mixture entering the Gay-Lussac tower varies slightly with the seasons. In extremely cold weather, 0.02 or 0.03 per cent is sufficient. In ordinary winter weather, with the thermometer ranging between 20 and 60 degrees, the sulphur dioxide at the end of the chamber system should be between 0.05 and 0.09 per cent. In summer, the desirable percentage is 0.09 to 0.15 per cent. As the seasons change gradually, the chamber operator experiences no difficulty in regulating the process so as to maintain in the chamber exit gas that sulphur dioxide percentage which the particular season demands.

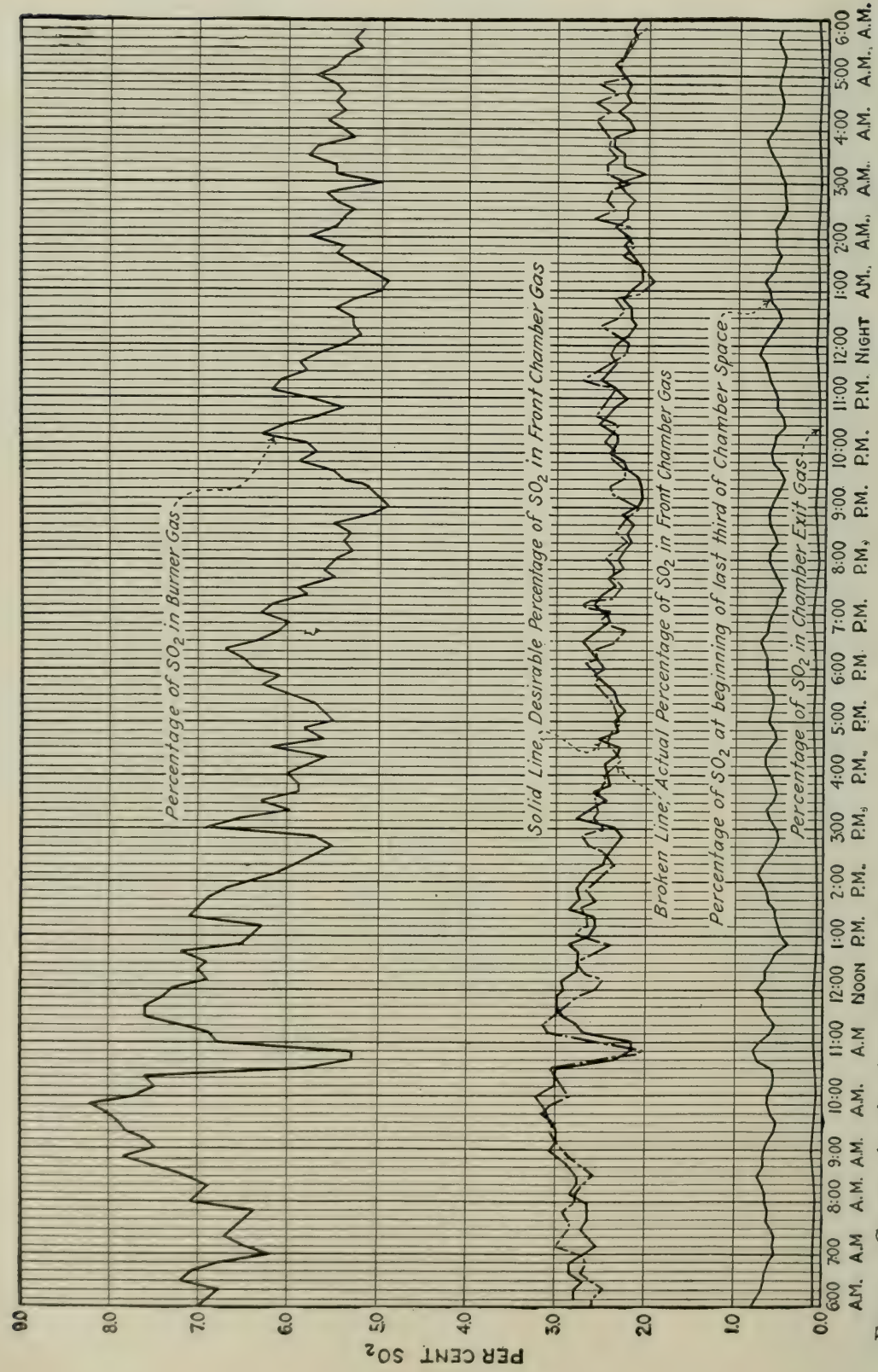
PER CENT SO_2
9.0
8.0
7.0
6.0
5.0
4.0
3.0
2.0
1.0
0.0
Percentage of SO_2 in Burner Gas
Solid Line, Desirable Percentage of SO_2 in Front Chamber Gas
Broken Line, Actual Percentage of SO_2 in Front Chamber Gas
Percentage of SO_2 at beginning of last third of Chamber Space
Percentage of SO_2 in Chamber Exit Gas
6:00 7:00 8:00 9:00 10:00 11:00 12:00 1:00 2:00 3:00 4:00 5:00 6:00 7:00 8:00 9:00 10:00 11:00 12:00 1:00 2:00 3:00 4:00 5:00 6:00
A.M. A.M. A.M. A.M. A.M. A.M. Noon P.M. P.M. P.M. P.M. P.M. P.M. P.M. P.M. P.M. Night A.M. A.M. A.M. A.M. A.M. A.M.

FIG. 2.—Curves showing how a fluctuating burner gas, properly controlled, yields in the intermediate chamber, and in the chamber exit gas, substantially constant SO_2 percentages.

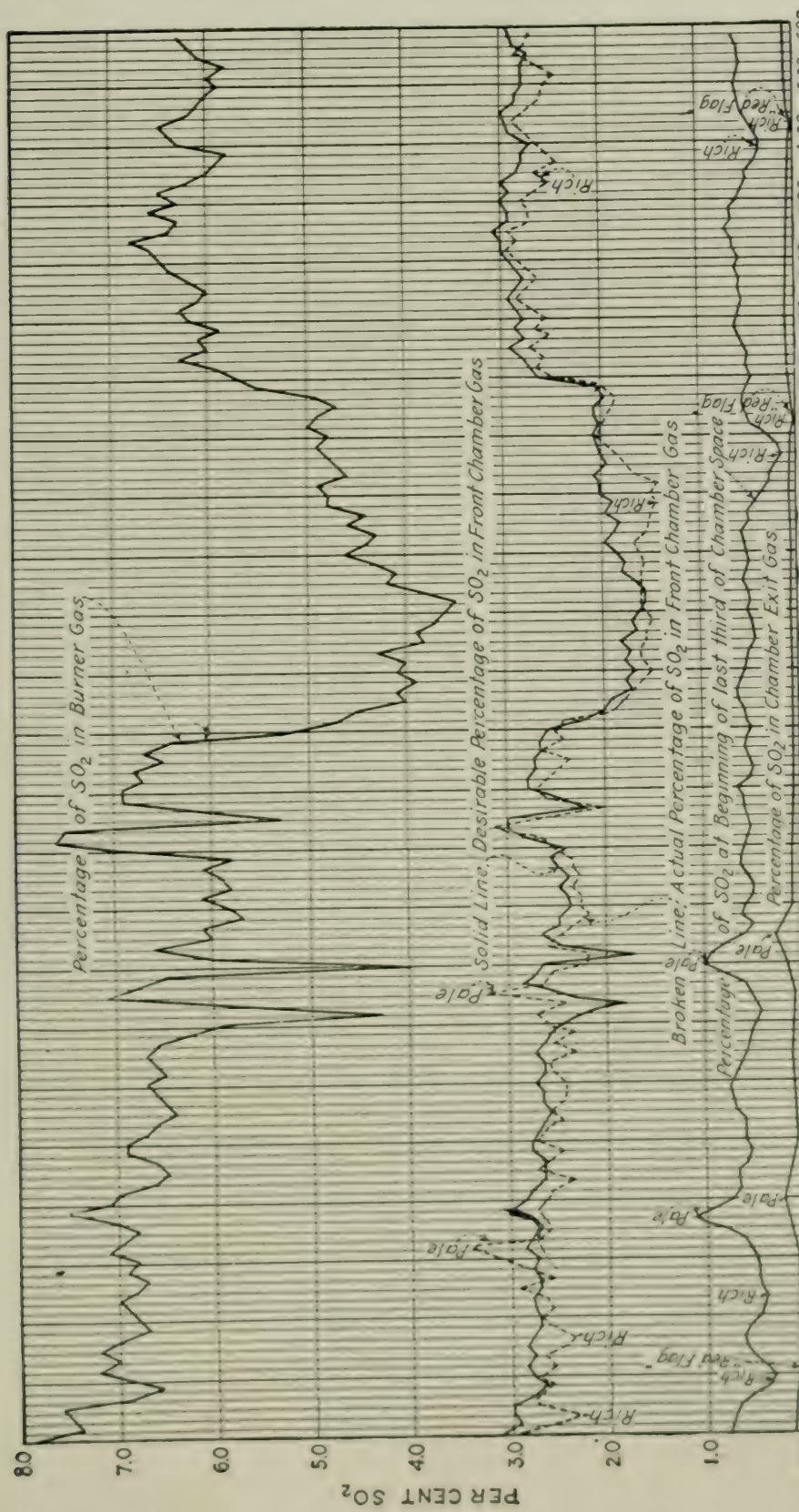
PER CENT SO_2
8.0
7.0
6.0
5.0
4.0
3.0
2.0
1.0
0.0
Percentage of SO_2 in Burner Gas
Solid Line: Desirable Percentage of SO_2 in Front Chamber Gas
Broken Line: Actual Percentage of SO_2 in Front Chamber Gas
Percentage of SO_2 at Beginning of last third of Chamber Space
Percentage of SO_2 in Chamber Exit Gas
Rich
Pale
Red Flag
6:00 7:00 8:00 9:00 10:00 11:00 12:00 1:00 2:00 3:00 4:00 5:00 6:00
A.M. A.M. A.M. A.M. A.M. Noon P.M. P.M. P.M. P.M. P.M. P.M. P.M. Night A.M. A.M. A.M. A.M. A.M.

FIG. 3.—Curves showing how a fluctuating burner gas, improperly controlled, yields a fluctuating SO_2 percentage in the intermediate chamber, and in the chamber exit gas, where constant SO_2 percentages are desired.

Fig. 2 shows graphically how a fluctuating burner gas, properly controlled, yields a fluctuating front chamber gas in its proper ratio to the burner gas, and at the same time a constant sulphur dioxide percentage at about the beginning of the last third of the chamber space, as well as a constant sulphur dioxide percentage in the gas entering the Gay-Lussac.

The curves in Fig. 2 were plotted from the results of tests made at ten-minute intervals at three of the testing points, and at hourly intervals at the entrance to the Gay-Lussac tower.

In contrast with the foregoing, the curves in Fig. 3 show how a fluctuating burner gas improperly controlled yields first a fluctuating front chamber gas out of proportion to the burner gas, and then a fluctuating sulphur dioxide percentage at the point in the latter half of the system where a uniform percentage is desired, and finally a fluctuating sulphur dioxide percentage in the gas mixture entering the Gay-Lussac tower, resulting in a " pale " back chamber, and red fumes escaping from the exit stack, alternately.

In this diagram it is plain that the lack of control of the process was evident from the tests in the front chamber, and that the " pale " back chamber, or the escape of red fumes from the exit stack could have been foretold an hour or more in advance. As a matter of fact, at the plant where the analytical method of chamber control was first installed, the chamber operators are able to predict a " red flag " about an hour or an hour and a quarter in advance of its appearance, and they are therefore able to correct any trouble at its very beginning.

Lunge's instructions for regulating the chambers are: " As soon as the last chamber turns pale, the cause of this must be sought for."[6] It is in keeping with the progress of modern industrial chemistry that for this obsolete counsel of a former generation we may now substitute: By means of the analytical method of control, be aware at all times of the condition of the *front* chamber. As soon as the *front* chamber becomes pale, ascertain to what extent it is pale, and apply more niter in the desired proportions. As soon as the *front* chamber becomes too rich in niter, ascertain the proportion of the excess, and reduce accordingly the amount supplied to the system.

[6] Geo. Lunge: Manufacture of Sulphuric Acid and Alkali; 4th ed. Vol. 1, p. 928.

DISCUSSION

Mr. MARSHALL: I should like to point out the fact, which is probably unknown to Mr. Fairlie, that for a number of years past some English manufacturers have been controlling the chamber process by comparison of the SO_2 percentages at various points in the set, also that in this country the Raschig modification of Reich's determination had been used prior to Mr. Fairlie's first published reference to his own modification.

The method proposed by Raschig is satisfactory when carried out with the usual analytical precautions, and I personally have found Raschig's method suitable for the accurate determination of the SO_2 contents of the gases on their way through a chamber set.

As the original publication of Raschig's method is of interest as a comparison with the modification proposed by Mr. Fairlie, I will read the part of Dr. Raschig's paper which covers the description of the modified Reich test.

The determination of sulphurous acid in chamber gases by the method commonly employed in testing furnace gases, namely, decolorization of a known volume of N/10 iodine solution in presence of starch, in Reich's apparatus, fails owing to the presence of nitrous acid. If, however, sodium acetate be added to the iodine solution, then the free acid produced is acetic acid, and the nitrous and sulphurous acid form sodium nitrite and sulphite respectively, which salts, as Divers has shown, do not interact. In presence of sodium acetate, then, the sulphurous acid in chamber gases, can be determined by means of iodine solution, and if subsequently the free acid be titrated, and the amount due to the acid liberated in the reaction between sulphurous acid and iodine be deducted, the amounts of nitrous gases can also be approximately ascertained, since these are converted into either nitrous acid or nitric acid, which liberate a corresponding quantity of acetic acid.

The CHAIRMAN: Is there any discussion on this paper?

Mr. BRADLEY: I would like to call attention to some results that were determined in connection with the determination of SO_2 in the presence of sulphuric acid. In smelter gases, where we don't have any nitrous fumes at all, we found very great discrepancies between the amount of SO_2 and SO_3, depending upon the method of sampling the gases, the handling of the tubes, and things of that kind. It is difficult to determine the presence of SO_2 in the pres-

ence of sulphuric acid. There was a paper on this subject published about a year ago that is very important.

The SECRETARY: Published by yourself, Mr. Bradley?

Mr. BRADLEY: No, I think by Nestel.

Mr. FAIRLIE: This is the first intimation I have had that the control of the chamber process by comparison of the SO_2 content at various points in the set had been used by English manufacturers. It is apparent that they have kept the method which they used secret, as I have been unable to find any published description of this method of chamber control.

From the brief statement by Mr. Marshall I cannot determine whether or not the method of analytical control has been developed in England as fully as I have developed it. Inasmuch as the statement made by Mr. Marshall may, at some time, bring up a question as to priority of use, I will state that although I made no published reference to my method until 1916, I was using my method in practice as early as 1910.

With reference to Raschig's modification of Reich's test, I have found that, for the purpose of controlling the chamber process by means of SO_2 tests on chamber gas and chamber exit gas, Raschig's modification is less reliable and less accurate than my own.

THE FIXATION OF NITROGEN.

By JOHN E. BUCHER.

Read at the New York Meeting, January 10, 1917

The herein described process for the fixation of nitrogen differs primarily from all those now in commercial use in fixing nitrogen in the form of alkali cyanides instead of in the form of oxides of nitrogen, calcium cyanamid, nitrides, or ammonia. It is further characterized by operating at very moderate temperatures, such as 900° to 950°C. so that it is not dependent upon cheap electric power, and, it can, because of this moderate temperature, be operated in iron retorts. It is of the utmost simplicity, uses iron which is the cheapest metallic catalyzer, and does not require pure materials such as nitrogen, but can use air or produce gas just as well. It requires no special apparatus and can hence be operated at once with what can be found in practically every manufacturing community. It does not require skilled labor to operate it, and it is preëminently a method which can be installed quickly in an emergency for the preparation of cyanides, ammonia and nitric acid.

These statements are based on a very large amount of chemical and engineering work, most of which was done three to five years ago, and which has appeared only in the form of patents from time to time. I have not heretofore published anything regarding any part of this work.

Meanwhile certain circumstances arose which made it necessary to abandon my work on nitrogen fixation and abnormal political and industrial conditions have affected our country as well as the rest of the world. I think that all thoughtful people agree that we are in no position to face any serious crisis which might suddenly arise.

For example, we are now practically in the midst of an alkali cyanide famine which is causing very serious hardships in a number of our industries and our Government officials estimate that

we would need 180,000 tons[1] of nitric acid per year in case of war with any first-class power and that it is of the utmost importance to have some ready means of getting this from atmospheric nitrogen so as not to be dependent on the hazardous expedient of importing it by ocean transportation in the form of sodium nitrate from South America.

The popular idea seems to be that it is necessary to have cheap hydro-electric power to provide such quantities of nitric acid together with a costly plant which would require considerable time for its construction. The data already accumulated in my work show that electric power is not necessary and that the process can be installed in a short time on any scale desired and at comparatively small expense. I had not intended to publish anything on my nitrogen fixation work for a few years more until I could complete some further important engineering work connected with it. The above considerations, however, led me to the conclusion that I could not in justice delay action any longer, and hence your kind invitation to present this paper was accepted with the hope of completing the work as opportunity offers.

HISTORY.

The fixation of nitrogen in the form of alkali cyanides is by far the oldest of all such methods, and its first period of great commercial activity dates from 1840 to 1847 when it terminated in failure. In January, 1839, there appeared an abstract of the work of Lewis Thompson[2] on "Improvement in the Manufacture of Prussian Blue" with the statement that he had been awarded the Gold Isis medal of the Society of Arts in the previous year for this work. After speaking of the wasteful process of producing cyanides from animal matter then in use, he says:

"Reflecting on these circumstances, it occurred to me that the atmosphere might be made to supply, in a very economical manner, the requisite nitrogen, if allowed to act on a mixture of carbon and potash under favorable circumstances. The experiment proved on trial to be correct, and in some measure exceeded my expecta-

[1] Landis, Transactions of the American Electro-chemical Society, 29, 83 (1916).

[2] Mechanics Magazine, No. 822, May 11, 1839, p. 92; also Dingler's Polytechnisches Journal, N. F., 23, 1839, 281.

tion; for the carbonaceous matter employed may be worked over again many times, and is even improved by each operation. I found it necessary to use iron, for a reason which will be apparent in the explanation of this process; when iron is not employed, a much higher temperature is required."

He ground two parts potash or pearlash, two parts coke and one part iron turnings into a coarse powder and heated the mixture in an open crucible in an open fire to a full red heat for about half an hour, stirring the mass occasionally. He obtained an abundant yield of cyanide which he converted into Prussian Blue.

I regard Thompson's remarkable work as the most basic that has ever been done on the fixation of nitrogen by the cyanide process. He discloses clearly the idea of using the nitrogen of the atmosphere and also states with the utmost clearness that iron is "necessary" in the process if it is to be carried out at a temperature short of "much higher" than a "full red heat."

Thompson's article soon led to very active discussions or investigations throughout the scientific world by some of the most noted investigators of the time, such as Berzelius,[3] Erdmann and Marchand,[4] Fownes and Young,[5] Langlois,[6] Rieken,[7] Delbrück[8] and Bunsen and Playfair.[9]

Practically all took the perhaps justifiable view that Thompson had not proved the fixation of nitrogen because it might have come from the coke, and some of those who did experimental work then prepared charcoal from sugar and heated it with alkali carbonate in a current of nitrogen. They all omitted the iron turnings and hence either obtained no cyanide, or only traces, or else had to heat it to a very high temperature. They finally concluded, however, that nitrogen could actually be fixed in this manner, but that the favorable conditions for fixation were not known.

[3] Jahresberichte, 21, 80 (1842).

[4] Journal fur Praktisches Chemie, 26, 412 (1842).

[5] Journal fur Praktisches Chemie, 26, 407 (1842).

[6] Ann. Chim. Phys., (3) 1, 117.

[7] Dingler's Polyt. Journal, 121, 286 (1851); also Liebig's Annalen, 79, 77 (1851).

[8] Jahresberichte, 1, 473 (1847).

[9] Journal fur Praktisches Chemie, 42, 392 (1847), and also Report of the British Association (1845).

Commercial work began in 1840 and in 1843 Newton[10] took out the first patent for the formation of cyanides from atmospheric nitrogen. Works were finally located at Newcastle-on-Tyne which regularly produced over one ton of yellow prussiate of potash per 24 hours, at a cost of 1.86 francs per pound, by drawing air down through retorts filled with charcoal containing potassium carbonate. These processes completely ignored Thompson's recommendation of the uses of iron and consequently had to be operated at a white heat. This caused poor yields, slow action, much loss of alkali, great expense in constantly renewing the refractory clay retorts which were speedily destroyed by the alkali and the process resulted in great loss of money and failure in 1847.

Since this numerous other attempts have been made to get cyanides from atmospheric nitrogen by methods which in the vast majority of cases did not use iron as a catalyzer. In 1881 to 1885 Victor Alder[11] of Vienna took out a series of patents in Germany and elsewhere; in the first of which he states that alkali carbonates can be converted into cyanides when heated to redness with carbon in nitrogen and that the process is essentially favored by the presence of metals such as finely divided iron, but that the use of iron is not an indispensable condition as the process succeeds completely even without its use.

In a second patent he states that the combination takes place copiously only in the presence of gases containing carbon (hydrocarbons, carbon monoxide, or a mixture of the two) in which metals that are able to transmit carbon, such as iron, manganese, chromium, nickel, and cobalt act extraordinarily favorably as will be shown below. These, together with his other statements made in his patents, constitute such a mixture of truth and falsehood that it is not surprising that failure resulted after six years' attempt to work the patents in Germany.

Thompson's original disclosure of the necessity of iron was finally so completely ignored or forgotten that Castner,[12] who used alkali metals instead of carbonates, stated that iron is "inert" while Acker,[13] who also used alkali metals, disclaimed iron as a "reactive" metal in the cyanide syntheses.

[10] Bertelsmann, "Die Technologie der Cyanverbindungen," p. 85.
[11] Bertelsmann, "Die Technologie der Cyanverbindungen," p. 90–91.
[12] U. S. Patent 577,837 (1897). [13] U. S. Patent 1,019,002 (1912).

In view of these complete contradictions and the complete failure of the fixation processes depending on the formation of cyanides it is hence not surprising that development took place along the lines of electrical fixation methods such as the arc and cyanamid processes as soon as the production of large quantities of cheap hydro-electric power was accomplished.

EXPERIMENTAL.

MAGNESIUM NITRIDE PROCESS.

The above hasty study of nitrogen fixation to cyanides did not seem very encouraging and I did not have access to the work of Thompson and Alder at the time. Accordingly some work was done with nitrides with a view to fixing nitrogen according to the following equations:

$$MgCl_2 = Mg + Cl_2$$

$$3Mg + N_2 = Mg_3N_2$$

$$Mg_3N_2 + 6NH_4Cl = 3MgCl_2 + 8NH_3$$

This process was electrolytic in nature and was designed to affect improvements in the ammonia-soda process. The magnesium chloride would be electrolyzed to magnesium and the resulting metal would be burned in the nitrogen from the towers to fix nitrogen as magnesium nitride. This later would be heated with ammonium chloride obtained from the mother liquors to produce ammonia and regenerate the magnesium chloride for the electrolytic stage. This would greatly modify the ammonia soda process by fixing its waste nitrogen, recovery of all the waste sodium chloride, elimination of waste liquors containing calcium chloride and the production of chlorine as a new product. It depends, however, on cheap electric power and would depend upon being operated in connection with the ammonia-soda works. As I wished to get a general nitrogen fixation process which was not dependent upon any locality or industry, this work was abandoned.

NITROGEN FIXATION WITH ALKALI METALS.

The decision was now made to test the chemical principles upon which the fixation of nitrogen to cyanide depended and it was decided to use free alkali metal at first to study the reaction:

$$2Na+2C+N_2=2NaCN+46{,}200 \text{ calories.}$$

An apparatus shown in Figure 1 was accordingly constructed of an ordinary ½-inch malleable iron pipe about 30 inches long, connected at both ends with a reducing coupling and with ⅛-inch inlet and outlet pipes.

This ½-inch pipe was then placed inside of a larger outer pipe which had tee and bushings at one end and a ring of asbestos board at the other. This apparatus gave a means of heating in a combustion furnace while the inner tube was surrounded by an atmosphere of hydrogen or nitrogen to prevent oxides of carbon from diffusing through the walls of the inner tube and forming sodium carbonate with the metallic sodium. Hydrogen diffuses into the inner tube so easily that, with a slow current of nitrogen and a two-foot section red hot, the exit gas at the end of the tube burned and contained about 200 c.c. of hydrogen per hour. Carbon monoxide diffuses very much more slowly and was detected with iodine pentoxide.

EXPERIMENT NO. 1.

The inner tube was then filled with lampblack containing substantially no ash and the whole heated in a current of hydrogen to a white heat, cooled, opened, and then 14 grams of sodium added at one end and a current of nitrogen passed into the inner tube. Reaction began slowly and continued for three days (25 hours) the current of gas being occasionally reversed to drive sodium vapor first one way and then the other.

The tube was cooled, opened and found to contain perhaps one-half gram of unchanged metallic sodium. The tube and contents were washed with water and gave 23.7 grams of sodium cyanide which is 79% of the theory based on the quantity of sodium used.

In another case the inner tube was charged with 80 grams of

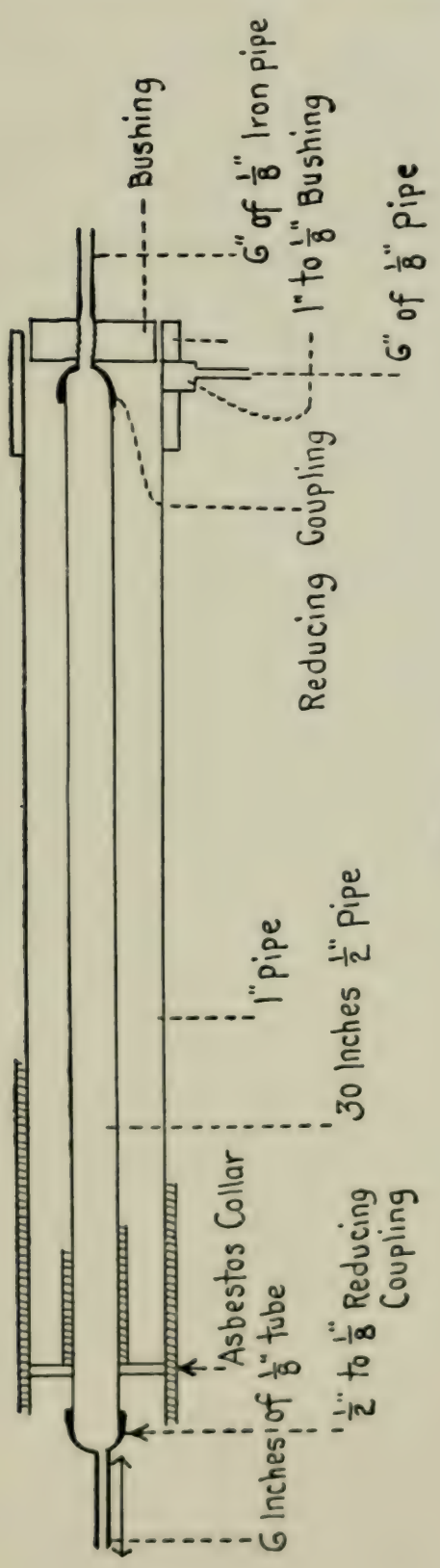
Bushing
6" of 1/8" Iron pipe
1" to 1/8" Bushing
6" of 1/8" Pipe
Reducing Coupling
1" Pipe
30 Inches 1/2" Pipe
Asbestos Collar
6 Inches of 1/8" tube
1/2" to 1/8" Reducing Coupling

FIG. 1.

electrode graphite, powdered to pass through a 100-mesh sieve, and ignited with a current of hydrogen for over one hour. Then it was cooled, 12 grams of metallic sodium were added and then it was heated in a current of nitrogen for 2½ days (about 20 hours) to redness as above, and 58% of the theoretical amount of cyanide was obtained. Quite a number of experiments of this nature were made and while the absorption of nitrogen was sometimes faster than at others, it was always a matter of hours. There is hence no question that cyanide formation is slow under these conditions.

EXPERIMENT NO. 2.

I now decided to try my idea that iron should act as a catalyzer notwithstanding the above assertions to the contrary. The same tube was hence heated as before while a current of nitrogen was passing, after being charged with 120 grams finely powdered alcoholized iron, 12 grams of ignited lampblack and after 7 grams of metallic sodium had been pushed into the charge.

When the tube got to a low red heat, absorption of nitrogen began so rapidly that a partial vacuum was formed and the water through which the exit gas bubbled started to rush back towards the hot iron tube and was stopped only by quickly turning on the nitrogen in a torrent. The absorption was practically instantaneous and there was no time to take observations. Only a small quantity of exit gas bubbled through the water and it must have been argon. The whole thing was finished just as soon as the requisite nitrogen could be passed in and 94% of the sodium was converted into cyanide. The absorption was exceedingly sharp and the end of the tube was entirely free from carbon and contained a core of porous iron of a bright silvery luster and so malleable that it flattened out under a pestle.

There were sintered globules of metal showing that the temperature must have risen hundreds of degrees higher inside of the tube than outside owing to the powerful exothermic nature of the reaction. This experiment shows with the utmost sharpness that iron is an exceedingly active catalytic agent in the fixation of nitrogen, that the reaction is powerfully exothermic, that we have a new method for the preparation of argon, and that the reaction gives a fine method of separation of nitrogen and the argon group in gas analysis.

We may now write the equation thus:

$$2Na+2C+N_2+iron=2NaCN+iron.$$

Suspecting that much of the slow absorption in Experiment No. 1 was due to the walls of the iron tube, the experiment was repeated by forcing a thin seamless copper tube into the inner ½-inch iron tube. This tube was charged with 4 grams of sodium and some ignited lampblack and heated in a current of nitrogen for four hours. No absorption could be noticed and on titration it was found that only 10 c.c. of nitrogen had been converted into cyanide. This shows very sharply the catalytic influence of the walls of iron tubes.

PURIFICATION OF IRON.

The above Experiment No. 2 and the equation show that while we are dealing with the fixation of nitrogen with sodium, carbon and iron, we are necessarily removing carbon from the charge and hence purifying the iron. Sodium in the form of solid, liquid or vapor, has frequently been added to iron and so has nitrogen. Sodium and nitrogen have even been added separately or alternately to the same mass of iron. But I have found no disclosure wherein any one even hinted at treating iron with sodium and nitrogen simultaneously so as to remove the carbon in the form of cyanide by virtue of the catalytic action of the iron itself.

This seems to be a novel process which is just as interesting as the nitrogen fixation itself. It depends upon a powerfully reducing action, while all large commercial processes depend upon the oxidizing action for the removal of carbon. It can not attack the iron after the carbon is removed as is the case in oxidation reactions. It will remove not only carbon, but also sulphur, oxygen and phosphorus. It suggests the idea of removing silicon and manganese by the oxidation processes even to the extent of over-burning the iron and then removing the remaining oxides, carbon, sulphur and phosphorus with sodium with or without the previous addition of carbon and that a Bessemer converter might be blown with nitrogen and sodium vapor. It also leads one to wonder whether with very cheap sodium it could not be made to apply to iron from ores very rich in phosphorus.

To further test these ideas for solid iron, a number of hack-saw blades were heated to redness in nitrogen and sodium vapor for a number of hours. They came out very soft and silvery in appearance and could not be tempered, thus showing that the carbon had disappeared and that the iron had acted as a catalyzer to purify itself. This removal of carbon, sulphur, and phosphorus must take place substantially instantly at the surface of the iron, and it is hence determined practically by the rates of the diffusion of these substances through the hot iron.

The removal of carbon from a piece of steel wire is so complete that when the latter is made the anode in dilute hydrochloride acid, it remains bright and there is not the slightest sign of carbon as the iron dissolves. If desired the surface of the iron can be purified quickly and the interior will remain in the form of steel. Many interesting topics arise regarding the electrical properties of iron thus purified, corrosion of iron and the coating the metal with tin, zinc, etc., but their discussion would require a separate paper.

PREPARATION OF METALLIC SODIUM.

EXPERIMENT NO. 3.

The general principles of physical chemistry regarding the reversibility of chemical reactions at once led me to consider whether the above reaction for the decarburization of iron might not be reversible and that it might hence be written:

$$2NaCN + Iron \leftrightarrows 2Na + N_2 + Carburized\ Iron.$$

You will at once notice that this is simply the equation for the casehardening of iron by means of cyanides which has been known for a long time. In fact, it is practically evident that when the iron takes the carbon from sodium cyanide, it must set free the other two elements, sodium and nitrogen, which do not recombine under the conditions at which casehardening takes place. This hence gives us a new method of getting metallic sodium based simply on the process of casehardening iron with the additional precaution of keeping away air or gases which would destroy the liberated metallic sodium. With all the practical use of

cyanogen compounds in casehardening, it is curious that no record of such a disclosure for preparing sodium could be found.

Accordingly some sodium cyanide was mixed with pure iron powder in an iron tube and heated. A considerable layer of metallic sodium was found in the colder part of the tube. Similar experiments with potassium cyanide gave metallic potassium quite readily.

The same thing happens when the cyanide is added to molten iron and this might be of special interest practically because the carbon could be removed from the molten iron by a current of air blown into the converter and cyanide then passed in again as before. The general method of operation would be very similar to that used in making water gas commercially.

I did not follow this up because I had no suitable means for testing the engineering features of such a process and the method of getting sodium by the electrolysis of cyanide with the incident formation of cyanogen, oxamid, oxalic acid and formic acid, which will be described later, seems to offer greater advantages than this new method.

NITROGEN FIXATION WITH ALKALI CARBONATES.

EXPERIMENT NO. 4.

Having now determined that iron or some similar element was an exceedingly efficient as well as essential catalytic agent in the fixation of nitrogen, I decided to test the following ideas:

At a given temperature which is quite high, sodium carbonate and carbon give the reactions

$$Na_2CO_3 + 2C \rightleftarrows 2Na + 3CO$$

which was formerly used for the preparation of sodium. It would hence seem that at moderate temperatures of say from 860° to 980° C. we might get traces of metallic sodium according to the equation. This free sodium should then react practically instantly with the carbon and nitrogen providing finely divided iron were present as has already been shown. If these ideas are correct, we should get an extraordinarily cheap and efficient method of nitrogen fixation, providing that the reaction velocity for the formation of metallic sodium vapor is high. This would mean that

as the sodium vapor disappeared to form sodium cyanide, more sodium would immediately be liberated, according to the law of mass action, to re-establish the equilibrium and the process would thereby become continuous, and we could represent it thus:

$$Na_2CO_3 + 4C + N_2 = 2NaCN + 3CO - 138{,}500 \text{ calories.}$$

The following two experiments support these views in the most striking manner:

EXPERIMENT NO. 4.

In this experiment a mixture of ten grams of finely powdered graphite was heated with five grams of sodium carbonate from 920° to 940°C. for fifty minutes in a current of nitrogen. A ½-inch copper tube was used in order to avoid the catalysis of iron walls.

At this temperature there was a very slow but steady evolution of carbon monoxide and some white fumes passed out of the tube with the gas current. These fumes imparted a steady but not very intense yellow color when they were led into a Bunsen flame. Upon cooling, a minute quantity of free metallic sodium was found in the end of the copper tube. This, hence, gives positive experimental evidence that the formation of metallic sodium from carbonate takes place according to the above equation at temperatures below 940°C.

The contents of the tube were lixiviated and tested for cyanides with iron salts in the usual way. No precipitate of Prussian blue formed even on standing for a few minutes and only a slight greenish blue color showed no more than a trace of cyanide to have been present.

A similar experiment was then made with thirty grams of finely divided iron and five grams of sodium carbonate in a slow current of nitrogen for 50 minutes at 920° to 1000°C. In this test the issuing gas did not impart the slightest color to the Bunsen flame, thus showing that no sodium was formed and that the carbonate is so slightly volatile as not to give a flame test under the conditions of the experiment.

EXPERIMENT NO. 5.

A mixture of ten grams of graphite and 10 grams of finely powdered iron with five grams of sodium carbonate were now heated in a ½-inch iron tube from 920° to 940°C. for fifty minutes in a current of nitrogen, just as in the preceding experiment in which no powdered iron was used. There was a steady flow of gas which burned with the characteristic blue flame of carbon monoxide. The escaping gases showed no fumes nor did they at any time show the slight yellow color.

The product showed a conversion of over 60% sodium carbonate into sodium cyanide and a very heavy precipitate of Prussian blue was obtained.

These two experiments establish the foundation for nitrogen fixation in the sharpest possible manner and they support the above quotation of Lewis Thompson's work in the most striking way. They show that at a red heat we can have no nitrogen fixation with carbon and soda ash alone, but, that when finely divided iron is added, we have an exceedingly efficient method.

These experiments which cost practically nothing and took only a few hours' time, were made in 1912 and they emphasize the utter inexcusability of those who quoted Thompson's invention and then ignored his positive statement about the necessity of the presence of iron for work at a red heat. They also show that Alder's above-mentioned statement that the fixation of nitrogen succeeds at a red heat even without the presence of iron, is entirely erroneous, and that his claim that "carbonizing" gases, such as carbon monoxide, must be introduced with the nitrogen if cyanides are to be formed abundantly has not the slightest basis in fact.

It shows at once that, by ignoring the work of Thompson, investigators and manufacturers were forced to work at such exceedingly high temperatures that the results could only be a ruinous destruction of apparatus and commercial failure.

The above facts are in harmony with Dr. Ewan's statement in his carefully prepared article in Thorpe's Dictionary of Applied Chemistry, Revised Edition, Volume II, 1912, page 196.

"A few temperature measurements which the writer made with a platinum-rhodium thermo-couple showed that potassium vapor

is first evolved from a mixture of potassium carbonate and charcoal at about 1350° and that the formation of cyanide takes place very slowly at this temperature, the potassium cyanide volatilizing for the most part."

These experiments, together with Dr. Ewan's temperature measurement, show why even the methods which used alkali metals *without catalyzers* were doomed to failure as well as those using alkali carbonate. It is of no use from the technical point of view to work on any method which involves conditions under which material suitable for apparatus will not stand up. I had this sharply in mind when I determined to get a cyanide fixation process which should be effective at say from 860° to 950°C. so that iron or copper apparatus might be used in carrying out the process. The addition of the iron catalyzer solved this problem, which I regard as the most important in my entire work, so completely, that, judging by Dr. Ewan's figure, the temperature of cyanide formation is lowered more than 500°C.

I made hundreds of experiments with powdered carbon in the form of charcoal, coke, lampblack, etc., and with sodium, potassium, cæsium, rubidium, and barium compounds in the form of carbonates and hydroxides to test various points under widely varying conditions and to get quantitative data for technical application.

Ultimately a series of tests was made with the powdered materials in horizontal iron tubes of varying sizes which ultimately reached six inches in diameter and ten feet in length. Time will not permit the description of these experiments and tests. I can only say that the best results were obtained with two-inch pipes, ten feet in length. In some of these cases equal portions of the solution, lixiviated from the cyanized charge, required the same number of c.c. of N/10 $AgNO_3$ solution, acidified with nitric acid, and of N/10 H_2SO_4 (with methyl orange) for titration.

These figures show all the alkali metals to be present as cyanides, and this is what I mean when I speak of cyanide of 100% purity. When such a solution is evaporated to dryness in vacuo, it is easy to obtain a residue directly which titrates for over 95% of sodium cyanide.

BRIQUETTING.

Having thoroughly established the general conditions for efficient nitrogen fixation with substances in powder form, the next thing was to work out engineering methods for handling the materials.

The inconvenience of heating powder in a current of nitrogen gas at a red heat naturally suggests briquetting. A study of the art reveals numerous examples of briquetting, and it was usually done with such substances as tar, pitch, resins, etc., while others used lumps of charcoal, wood, and like substances to attain the desired porosity.

Desiring to avoid the use of such foreign substances, I first prepared briquettes by heating the mass of sodium carbonate, coke and iron to the melting point of soda ash in the absence of air. The pasty powder upon slight pressure becomes compacted and yields briquettes which are very hard and compact upon cooling. I have found such briquettes to be quite active, and they gave cyanide of 100% purity upon being heated for ten minutes in a current of nitrogen.

This is a fascinating process since the briquettes would already be at a cyanizing temperature just as they are formed. It would, however, mean the solving of unique engineering problems, hence I did not follow it further at the time.

The experience with the red-hot briquettes lead me to conclude that perfectly satisfactory briquettes could be made by adding hot water to the mixed charge of coke, soda ash, and iron, and making use of the solubility curves and transition points of the phases of sodium carbonate as shown approximately in Figure 2.

The curve at a glance shows that the mass should be mixed with the water at as near the maximum temperature of 104.75° as convenient. We shall then have a mixture of monohydrate, $Na_2CO_3.H_2O$, coke, and iron moistened with a hot, saturated solution of sodium carbonate. These phases can not change on cooling until the temperature reaches about 35° C. at which the crystals would take up all the water and we would later have a dry hydrate, $Na_2CO_3.10H_2O$, as far as the water added would permit.

In other words, there is here a range of fully 70° C. through which the mass might cool before the briquetting operation would be interfered with. These predictions were realized in an exceed-

ingly satisfactory manner, and many tons of briquettes were prepared without the slightest trouble. At first an ordinary concrete mixer without steam jacket was used. The hot water and the heat of hydration of the soda ash brought the temperature (work done in the summer) high enough to give fine briquettes, but as the temperature was not much above the transition point, the whole mass had a tendency to set like plaster of paris if the operation were delayed.

Later a steam-jacketed kneading machine was used with the utmost convenience so that the full length of the monohydrate curve BC was available before cooling could stop the process. The hot, dough-like mass was briquetted with an ordinary small power-driven

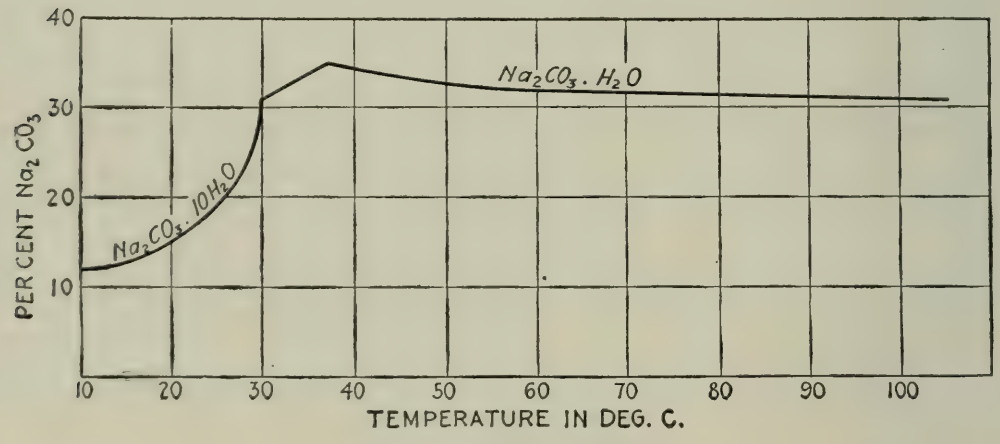

FIG. 2.

meat chopper having a cylindrical worm with a steel disc of 3½ inches diameter and having 37 circular holes of ⅛ inch diameter.

The knife was fixed so that the briquettes were cut off at about one inch length. The efficiency of these small machines was surprising. For example, the 9-gallon steam-jacketed kneading machine gave 7200 lbs. of briquettes per 24 hours with 15 minutes to the charge, and it could just as well have been run at 5 minutes to the charge to give 21,000 lbs. of briquettes per 24 hours.

The meat chopper, which was smaller than that often seen in a city market, easily gave 5,000 lbs. of briquettes per 24 hours with inexperienced help, and I have been able to force it to three times this capacity for a short interval.

Thus even a few of these machines not much larger than toys

would give a very large yearly output. Of course, a large brick-making machine would give a very large output commercially.

These briquettes should be dried rapidly so as to get them hard and free from dust. Hot waste gases are very satisfactory for such drying. A commercial bake-oven would dry many tons per day or a dryer can be constructed out of brick and iron very quickly. If the briquettes cool before they are sufficiently dry they will harden, and, on further standing, will fall to powder as one might expect when the bulk hydrate, $Na_2CO_3.10H_2O$, forms to break them up just as freezing water may break a spongy, brittle substance. I have dried briquettes in the hottest part of the Bunsen flame in less than two minutes.

The powder was prepared for this work on a commercial scale by grinding iron scale (magnetite, hæmatite, etc., will do just as well) to a 100-mesh powder in an iron ball mill with manganoid steel balls; then an equal weight of coke previously ground in a similar way to 100 mesh was added and the grinding continued for perhaps an hour, then the soda ash was added and the grinding continued for five minutes or even less.

The iron or iron scale and coke, being solids, must be mixed with the utmost thoroughness, but the soda ash becomes mobile by addition of water in briquetting as well as by fusing at 860° C. in the furnace so that no great care is necessary in mixing it with the other two constituents. Briquettes thus prepared with iron scale gave as high as 28% of NaCN in the heated charge. Thus it is shown that we may start with iron oxides instead of metallic iron. In the first run we may thus produce finely divided iron which is in very fine condition for subsequent runs.

I will hereafter, for convenience, speak of the surface of carburized iron which is in close contact with the ground carbon as the "catalytic solution surface." For cyanide formation it is only necessary to have nitrogen, sodium carbonate, and such a catalytic solution surface in contact simultaneously. It is hence evident that sodium carbonate in too large quantity will flood the solution surface so that the nitrogen can not get at it readily. This will smother the reaction so as to make it sluggish, and it is hence evident that there must be a percentage of soda ash which will give maximum commercial efficiency.

The three following experiments were made to give a rough quantitative idea of this factor: They were made with 50 lbs.

of alcoholized iron and with 50 lbs. of pulverized coke ground for five hours in the ball mill and the mixture was then briquetted with three different proportions of soda ash.

EXPERIMENT NO. 6.

In this case the iron-coke-soda ash was briquetted in the ratio respectively of 2:2:1 so as to give a 20% soda ash briquette. Then 150 grams of these briquettes were heated in a one-inch iron pipe in a current of nitrogen at a temperature from 600° to 1,000°C. The reaction took place briskly and the issuing gas burned finely. The briquettes were cooled, lixiviated and titrated as in the way described above and they were found to contain 13.7% of actual NaCN corresponding to 85% of the total alkali metal in solution. For brevity, I shall speak of such cases as 85% purity.

EXPERIMENT NO. 7

Here the same materials were briquetted to make 1:1:1 iron-coke-soda ash ratio and 150 grams were heated as before in the one-inch horizontal iron tube from 600° to 1030° C. for 30 minutes. The gas evolution was considerably slower than in the preceding Experiment No. 6. The resulting briquettes contained 20% of actual NaCN of 95% purity.

EXPERIMENT NO. 8.

In this case the iron-coke-soda ash mixture was made into 1:1:2 briquettes which contained 50% of soda ash, and 150 grams were heated in the same horizontal iron pipe in the nitrogen current for one hour at 600° to 1090°C. The combustible gas evolved sluggishly during the entire time, and the mass finally contained 14% of actual cyanide of 80% purity.

These three experiments show that the 20% soda ash briquettes were quite reactive, the 33% ones intermediate, and the 50% one rather sluggish, so that they had to be heated longer and to a higher temperature in order to get the poorer result. These experiments rather indicate that with either larger or continuous acting furnaces it should be easy to get cyanized briquettes to contain at least 30% to 35% of actual NaCN and this predic-

tion was later verified. The experiments also clearly show the loss of alkali compounds by volatilization during the run.

If the briquettes do not completely fill a horizontal pipe, or, if they sag somewhat, as inevitably they must as soon as they become plastic when soda ash melts, then the nitrogen will tend to pass through the upper channel rather than through the briquettes. This is further aggravated by the counter current of carbon monoxide which is formed in the reaction.

This factor is not very serious in a ½-inch pipe or even a one-inch pipe, but it must become more serious with larger diameters. The following two experiments give conclusive data on this point.

EXPERIMENT NO. 9.

I heated 25 lbs. of the same 2:2:1 briquettes as were used in the one-inch pipe in Experiment No. 6, in a horizontal 6-foot piece of 6-inch iron pipe and kept the temperature from 1000° to

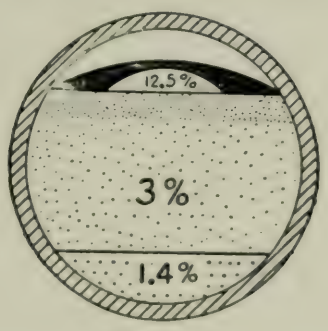

FIG. 3.

1080°C. for one hour and twenty minutes while a current of nitrogen was passing.

Upon cooling it was found that the plastic charge had sagged one-half inch in the upper part of the tube to produce a crescent-shaped channel as shown in Figure 3.

The entire charge averaged only 3.5% of actual NaCN of very low purity as against 13.7% cyanide of 85% purity under the far milder and shorter treatment of the same sample of briquettes in the one-inch horizontal pipe in Experiment No. 6. A vertical section at about the middle of the charge was also tested and it showed 12.5% cyanide in a narrow top layer, then 3% in the broad intermediate layer, while the bottom layer showed only 1.4% NaCN. This is also shown diagrammatically in Figure 3.

EXPERIMENT NO. 10.

The result in the preceding experiment showed that increasing the size of a horizontal pipe to 6 inches, when the current of nitrogen passed into the end, completely ruined the process commercially. If this theory is correct, then this damage could be avoided entirely by placing the pipe vertically. This was done with the same 6-foot pipe of 6-inch diameter, and as I had no more of the 2:2:1 briquettes on hand, we used the more sluggish 1:1:1 briquettes as a 50-lb. charge.

The pipe was heated from 1000° to 1090°C. for only one hour and ten minutes with a rapid current of nitrogen passing. This gave a massive column of yellow flame which at its maximum arose to a height of 4 to 5 feet above the mouth of the vertical 6-inch shaft.

On cooling, the top of the charge was found to contain 24% of actual NaCN while the bottom contained 20%, giving an average for the entire charge of 22%, and showing that over 10 lbs. of cyanide must have been produced in this run.

These two spectacular runs on a semi-commercial scale show at a glance that my process can be absolutely ruined by simply placing the retort horizontally in the furnace insead of vertically.

The same thing was also shown with longer and larger pipes in which the charges were on a scale of hundreds of pounds of briquettes in each run.

EXPERIMENT NO. 11.

This run was made in the same vertical pipe of 6-inch diameter which had been used in the preceding Experiment No. 10. It was made to test the volatility of the alkali compounds quantitatively. The charge happened to be 55 lbs. of briquettes consisting of 22 lbs. of iron scale, 24 lbs. coke and 9 lbs. of soda ash. The heating continued for two hours at 1000° to 1100°C.

Upon cooling, the top layer of briquettes was found to contain 28% of actual NaCN of 87% purity. Passing down, the other sections showed respectively, 16%, 14%, 12%, and 10% of actual NaCN and the purity of the four sections varied from 90% to 93%.

The lower section contained a central core of moderately firm briquettes whose content was 12% of NaCN of 100% purity. This core was surrounded by an annular section which had fallen to powder because its content of 7% of sodium cyanide was no longer enough to hold the particles of iron and carbon together.

These results are shown diagrammatically in Figure 4:

The results show exactly the general results we should expect in an experiment of this sort where we have heat passing in through

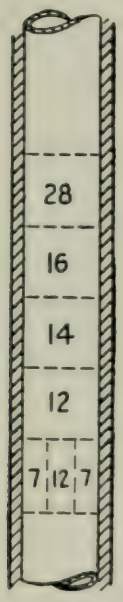

FIG. 4.

the walls of the tube and a rapid gas current sweeping through the hot briquettes exposing a large surface for the evaporation of alkali compounds. The endothermic reactions in this case intensify the effect.

This experiment at once suggests two methods of solution, both of which I have carried out successfully in practice. The first would be to make the briquettes so active that they could be cyanized at lower temperatures, thus reducing the loss by volatility very greatly and by making other suitable changes. The other solution would be a continuously operating furnace, and here great volatility would be a most valuable feature.

ACTIVITY OF BRIQUETTES.

The two following experiments were made with 25 grams of briquettes heated in each case in a current of nitrogen in a ½-inch iron tube which was laid horizontally into the heat zone of the furnace.

EXPERIMENT NO. 12.

In this case 2:2:1 iron-coke-soda ash briquettes were used. They were heated from 710° to 920° C. in 13 minutes. There was an exceedingly rapid evolution of carbon monoxide which died down to almost nothing in 6 minutes before a temperature of 900° was reached. The product contained 15.2% of actual NaCN of 92% purity. We may, hence, conclude that 2:2:1 briquettes should give over 15% cyanide of over 90% purity in ten minutes at temperatures below 920°C.

EXPERIMENT NO. 13.

In this case the alkali was washed out of the briquettes and soda ash then added directly to the moist filter cake of iron and coke so as to make 1:1:1 iron-coke-soda ash briquettes. These briquettes did not give the rush of carbon monoxide which was noted in the preceding experiment, but there was a steady evolution of gas during most of the 28 minutes while the tube was being heated from 620° to 920°C. There was still some action when the heating was stopped.

The resulting briquettes contained 30% of actual NaCN and the purity was 87%. This very satisfactory experiment shows that we can get over 30% cyanide of 87% purity in a batch furnace below 920°C. in less than one-half hour's heating. We may, hence, conclude that an efficient nitrogen fixation takes place below 920°C.

Another experiment was made with briquettes from the same lot of 2:2:1 iron-coke-soda ash briquettes that were used in Experiment No. 12. They were heated for ten minutes from 560° to 820°C. and gave 11.5% of actual NaCN of 61% purity. This shows that even below 820°C. we get considerable quantities in a few minutes. The process can, hence, certainly be carried out at very reasonable temperatures.

This could easily be carried out in copper tubes which do not oxidize easily. It also shows that at mines the cyanide could be lixiviated and the moist filter cake could be treated with soda ash in the kneading machine, briquetted, and used over again.

CONTINUOUS FURNACES.

The second way of overcoming the loss of alkali noted under Experiment No. 11 is to have the charge moving continuously, preferably by gravity; this would keep the final concentration of alkali in the briquettes just the same as at the beginning, because, whatever distilled from the heat zone would necessarily have to return to it and, hence, would be of advantage in insuring contact rather than a disadvantage.

This type of process would have great advantages in many other ways, some of which will be pointed out. The process would act very much like a blast-furnace but it would differ in yielding a plastic product instead of molten products.

The first experiment was carried out with an 8-inch iron pipe, 8 feet long, placed vertically in a furnace and connected by means of a tee with a worm conveyor immediately below the furnace. Everything worked well for a while until the hot plastic briquettes got into the conveyor and hardened so that it could not be turned. This was rather what had been expected. Therefore the experiment was modified by using a tube 14 feet long so that the worm conveyor could be placed about 4 feet below the bottom of the furnace.

Figure 5 shows the principle of the arrangement.

Here the descending, incandescent, plastic briquettes would cool by the ascending current of nitrogen, and by the air outside. The cooling was made more thorough still by using a current of water and a piece of cloth wrapped around the pipe below the heat zone. In this way the briquettes became cooled and hardened so that they passed through the conveyor in about the same physical condition as that in which they entered the furnace.

The hardened briquettes, however, stick in the pipe so that hammering may be necessary to move them, and at times when the pipe was rough it gave much bother. This can be helped in various ways, such as making the entire pipe conical, or by making only that part below the heat zone conical, by making the briquettes

larger, by feeding briquettes poor in alkali in the annular ring next to the wall of the pipe, or even pieces of coal or coke might

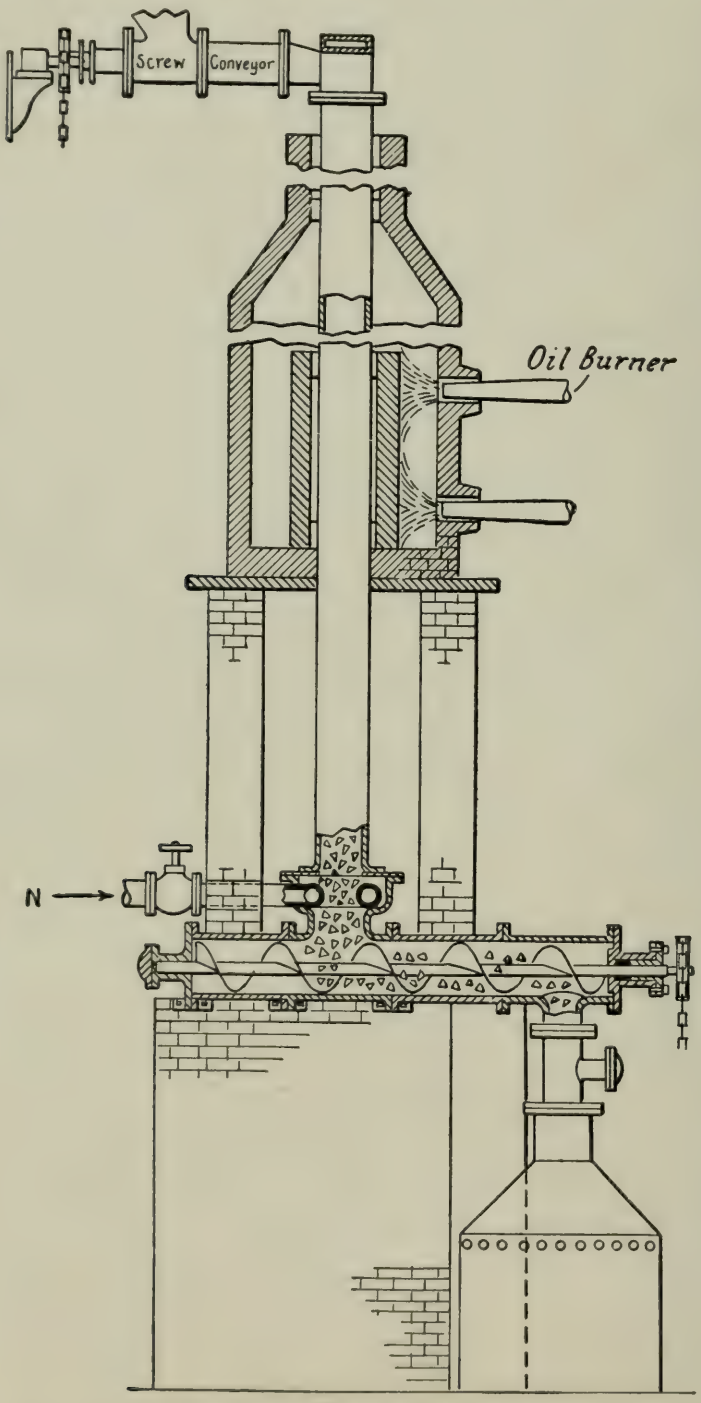

FIG. 5.

be fed into the outer annular space while the core of the charge contained the briquettes richer in alkali, or, the cooling zone might

be a larger pipe than that used for the heat zone as shown in Figure 6.

A complete solution would no doubt be obtained by making the cooling zone so wide that the hot cyanized briquettes would have

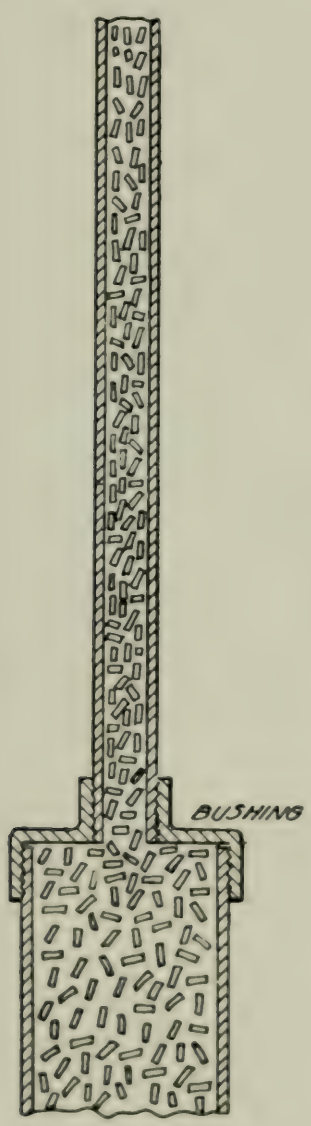

FIG. 6.

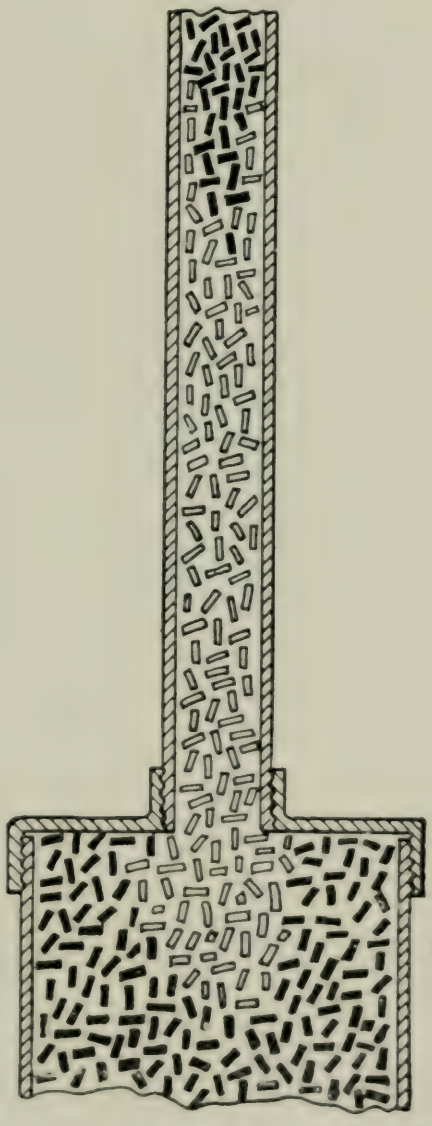

FIG. 7.

to cool below the solidifying point of the sodium cyanide before they could come in contact with the iron walls.

The idea is well shown in Figure 7, where the hot plastic briquettes are shown not shaded while the cooler hardened briquettes are shaded. I have never had the hot plastic briquettes stick in a furnace operated according to my direction; the sticking

always took place after they had hardened. Also, solid briquettes never stick in a properly operated furnace.

This very simple principle shown diagrammatically in Figure 7 can hence scarcely fail to remove this difficulty absolutely. There is a slight tendency to stick at the top of the heat zone where the briquettes are just softening, but I have also been able to obviate this with the utmost ease by observing certain conditions.

In some of the work I made tests in a vertical iron pipe 22 feet long and 8 inches in diameter. This required 500 pounds of briquettes to charge it and then, in some tests, 3,000 lbs. were fed in a 24-hour run.

The furnace was constructed on an improved principle as compared with that shown in Figure 5. The furnace consisted essentially of a double chimney with a thin partition separating the two compartments. An oil burner flame was directed vertically down one flue and the flame then led around baffles into the other flue which contained the 22-foot pipe. The flame then passed up around the pipe and by controlling the draft at the top an effective furnace was obtained.

I further led a number of air jets into the main flue in such a way that the air oozed through long, narrow slits into the burning gases. In this way a reducing combustion could be maintained in the heat zone and the entire length of pipe in the furnace could be heated to a perfectly uniform temperature when desired. The whole furnace was within a derrick-like structure high enough so that burned-out pipes could be hoisted out mechanically without cooling them, and new ones inserted. Considering that these were simply first crude attempts, the furnaces worked surprisingly well. I believe that they are practical and that a gas producer would do the work far more easily but, unfortunately, I did not have one, hence the oil-burner furnaces.

AN ELECTRIC FURNACE.

When the price of yellow prussiate of soda and red prussiate of potash began to rise and ultimately reach values of $7.00 and $28.00 respectively, for each pound of nitrogen they contained, I decided to construct two types of electric furnace which depended on the principles I had in mind for years, in case of emergency either at the mines or for these chemicals.

It was self-evident that they would work and that they could be installed in a few hours where any sort of commercial current is available. They depend simply upon using iron as a resistor and then keeping away oxygen so that they can not burn out.

The first one was simply a piece of 1½-inch galvanized iron pipe such as is used on buildings as a water conductor. Two strips of copper were clamped about the ends for leads and the whole tube set vertically in a chimney of brick which were piled on the floor to give an opening 9 inches square. The space between the galvanized pipe and the brick was filled with magnesia asbestos and the furnace was complete.

It was filled with briquettes and the currents of nitrogen and electricity were turned on. The zinc burned off like a flash and in a very few minutes there was a steady, uniform heat in the briquettes and a fine flame of carbon monoxide. The transformer only gave 350 amperes and I had no means of measuring the voltage, which was probably not over 2 or 3 volts. The resulting briquettes contained over 22% NaCN.

Wishing to carry out this exceedingly satisfactory experiment quickly on a larger scale and being unable to get a suitable transformer, I purchased a small 15-kilowatt welding machine for the emergency. These machines are rated at 2 or 3 volts for the secondary current and only 50 to 75% efficiency because they are light and intended only for intermittent service.

The furnace was now made from a piece of ordinary 4-inch iron pipe, 6 feet in length. This was thrust through an iron drum about 4 feet high and thirty inches in diameter with round holes for the iron pipe. The leads from the transformer were connected to the pipe by strips of copper which had a cross section of 3 inches by ¾ inch. The drum was then filled with magnesia asbestos and the whole furnace was complete. Then an 8-inch to 4-inch reducing coupling was provided at the bottom of the 4-inch pipe and this in turn connected to a 4-foot length of 8-inch pipe which was closed with a cap at the bottom. A hole for a one-inch pipe was then drilled through the top of the reducing coupling and at one side there was a stuffing box for the ¾-inch iron rod which held up the charge. The other end of this rod was supported by a piece of one-inch iron tube which served as nitrogen inlet as well.

Figure 8 shows the furnace diagrammatically.

A piece of fire brick was now cut and a round piece of perforated fastened to the top as shown in Figure 8a. This was lowered into the furnace to support the column of briquettes.

The current available was not over about 9 kilowatts and this was turned on. After several hours the pipe had not become quite

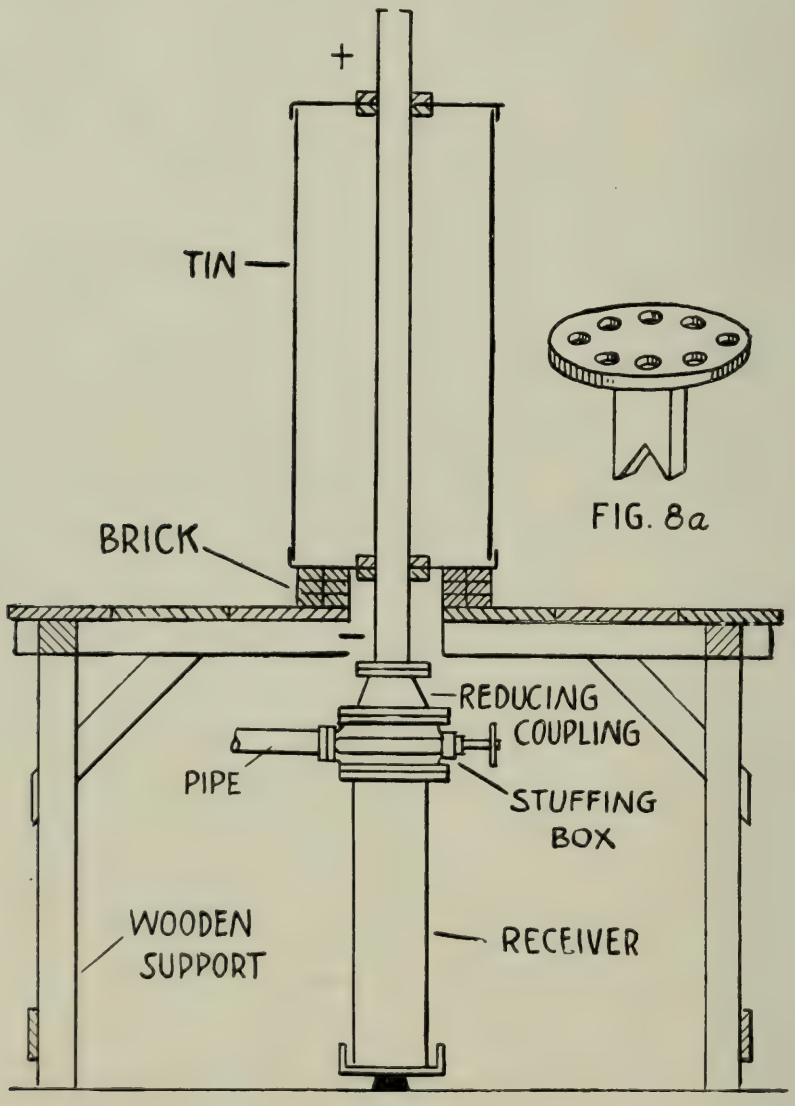

FIG. 8a

FIG. 8.

red hot but the machine and the copper leads were unduly heated. Accordingly a number of buckets of snow and ice were thrown over the leads and the welding machine. When the clouds of steam cleared away it was found the iron tube was now at a good low red heat and it became hotter gradually.

The treatment with snow and ice was continued through all

the runs as it raised the efficiency by lessening the resistance of external circuit. Briquettes were now added and a fine run made, but it took three hours because the temperature was not as high as it should have been.

Even so, I had no trouble in getting charges which gave 25% of actual NaCN consistently. When the runs were finished a cap was turned on the top of the 4-inch pipe and the iron rod supporting the charge withdrawn.

The entire charge dropped into the lower 8-inch pipe. The iron rod was now pushed back, the top opened and a new brick support lowered into the furnace and 15 pounds of briquettes poured in.

The whole change took less than two minutes and the electric current was not shut off. The top was now closed again without interrupting the currents of electricity or nitrogen, and the 8-inch pipe removed and a new pipe substituted and the run proceeded as before.

The current was kept on this for about three days and a considerable number of runs made. The iron drum should be of thin metal to reduce induction currents. It was so satisfactory in every way that I considered it unnecessary to run it any longer. This furnace, hence, avoids sticking of the charge and if the drum is kept tight, or a little nitrogen or reducing gas passed into the magnesia asbestos, it will be impossible for the pipe to be destroyed by oxidation.

By using a large number of pipes in series it would not even be necessary to provide a transformer, since we could allow a drop of a few volts for each pipe. If one has a three-phase current of, say, 125 volts at 120 amperes, it is possible to modify the furnace shown in Figure 8 by simply setting a few fire-clay tiles 2 feet high by 12 inches inside diameter around the 6-inch iron pipe which I used in this case. I allowed 3 pieces of ⅛-inch iron wire each 30 feet long shaped as shown in Figure 9 to hang down into the annular space between the tile and the pipe.

There were 3 pieces of fire brick at the bottom to keep the wires spaced from each other and from the iron pipe. The three segments were then used with Y-connection and I was thus enabled to get 18 H.P. into the furnace instead of 12 by a single phase.

A slow current of nitrogen, carrying some gasoline vapor, was passed into the annular space to prevent oxidation of the thin,

white, hot wire. This worked finely and gave over 30% of NaCN in the briquettes used. The wire, even with this imperfect construction, lasted 9 hours while it probably would not have lasted 9 minutes if air had been admitted.

On a large scale this furnace would be very simple since we would use only a single phase to each furnace and also use the furnaces in series and we would soon have to use, say, one-inch iron rods for the resistors even with fairly high voltages such as 250 to 500. This entirely practicable type of furnace would probably be

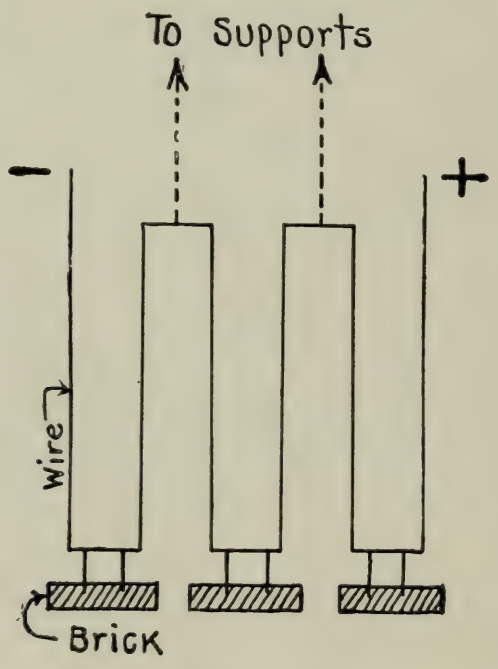

FIG. 9.

less desirable than that described already where the pipe itself is the resistor, but, in small scale work it can be connected at once to any city current without the expense of loss of time in waiting to have special low voltage transformers made.

The resistance of iron increases probably five to six-fold in heating from the room temperature to the cyanizing temperature, and varies with carbonization, etc. I have not been able to make strictly accurate observations, but in all of my work I found that the following formula gave me sufficiently accurate results so that the field rheostat regulation gave me entire control over the current.

$$\text{Amperes} = \frac{500VW}{L^2}$$

where V was the voltage used, W the total weight of iron in pounds, and L the total length of the conductor. I found it to hold

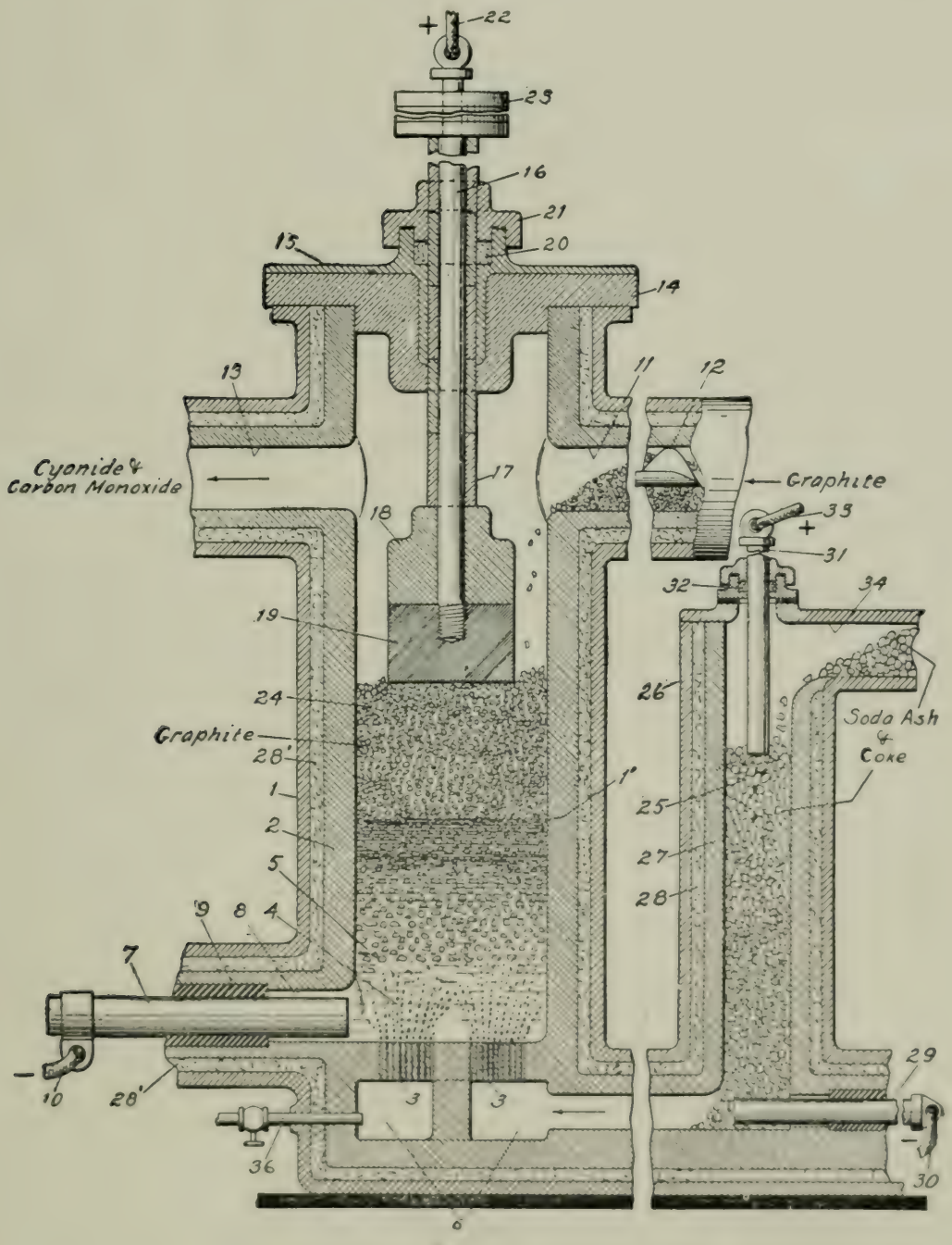

FIG. 10.

for 1/16-inch wire, and for rods ½ inch in diameter as well as for pipes from 1 to 4 inches in diameter.

These electric furnaces are not what I should intend to install

if I were asked to put up a large plant, but they are what I would put up for an emergency plant (to which I will again refer) because I have personally worked through every phase of these processes and nothing whatever is left to chance or guess-work. The time factor would in such a case be more important than the greater economy and speed which I feel can almost certainly be obtained by internal electrical heating where the briquettes themselves are the resistor.

While we are speaking of the application of electrical heating with solid catalytic solution surfaces, it may also be well to mention the experiment with molten iron.

Figure 10 shows the apparatus and is almost self-explanatory. It represents a cylindrical furnace with basic lining and a perforated bottom like a Bessemer converter. It contains molten iron into which coke or graphite fragments are deeply pressed. The graphite column acts as electric resistor to produce the heat and, while in the bath, it serves both to retard the gas bubbles so as to insure sufficient contact and to keep the iron saturated with dissolved carbon.

Sodium vapor and nitrogen are blown into the bottom of the furnace and cyanide distills out at the top with the argon. The linings I used in the short time of the run stood better than I had expected. If the lining will stand sufficiently in actual work then I feel that ash in the carbon will be the other serious factor since the method of getting ashless carbon mentioned hereafter will not apply, but there are, however, other methods of solution.

RATE OF HEAT PENETRATION.

In considering the technical application of these inventions, the rate of heat penetration into the briquettes becomes very important. The following experiment enables us to form some idea of the magnitude of this factor.

EXPERIMENT NO. 14.

In this run a 4-inch vertical pipe of iron was used with a pyrometer inserted in the center of the 8-pound charge of the 2:2:1 iron-coke-soda ash briquettes through a ½-inch iron tube. The whole was placed in a cold furnace and a powerful oil flame used in the heating. By the time the outer pyrometer read 1030°C.

the inner one in the charge read only 150°C. After 40 minutes more the outer one reached 1080°C. while the inner one only registered 880°C. In another twenty minutes the inside temperature read 1010°C. while the outer remained constant at 1080°C. The heat, hence, flows in slowly and the rise is influenced by many factors of which the heat absorption of 138,500 calories is one. The result was 17% of NaCN of 99% purity in the briquettes.

The experiment gives the valuable data which enables us to say that it would take quite a while to heat a 6 or 8-inch column of briquettes to a uniform temperature. It also seems to confirm the very probable idea that the charge will conduct much better after it is partially heated than when it is cold. The importance of both these observations will be shown later.

INTERNAL ELECTRICAL HEATING.

Experiment No. 14 shows that this factor would seriously retard rapid working in cyanide formation with large masses where the heat had to penetrate in from the outside. It would be a question of hours in large tubes. If we could use the charge directly as a resistor and thus generate the heat within the briquettes themselves, the heating would be reduced from hours to minutes with proportional saving of radiation losses and increase of output. Experiments along this line are very hopeful and wonderfully interesting, and I think no similar thing has ever arisen in electrical furnace work. To adequately discuss the very important results already obtained would require more time than is available for this entire paper and I am not yet at liberty to disclose the most important of them.

I am, hence, compelled to dismiss this with the following general statements: The briquettes contain an insulator soda ash which makes them practically non-conductors at the room temperature, but the conductivity increases very rapidly until at cyanizing temperatures it may easily increase 3,000-fold.

It is, hence, only necessary to heat the charge to start the current or to put the briquettes into a hot furnace. I have heated a charge in a glass tube with the current in a few seconds so that the entire run was finished in less than one minute with abundant yield of cyanide, and I have also heated charges of many pounds in a few minutes.

This suggests wonderful possibilities and shows that it is an entirely different proposition from that discussed in the case of the electric furnace shown in Figure 8.

AIR INSTEAD OF NITROGEN.

Having now shown how to fix nitrogen with simple apparatus with pure nitrogen, I desired to get away from the use of pure nitrogen so that my process would not be tied up to ammonia-soda or similar works, where nitrogen is a waste product. Also I did not wish to be compelled to put up a liquid air or other plant in order to get nitrogen. I suspected from the fact that carbon monoxide is formed in large quantity in the process, that a little more would do no harm under the right conditions. This suggests at once the use of producer gas in place of pure nitrogen or even

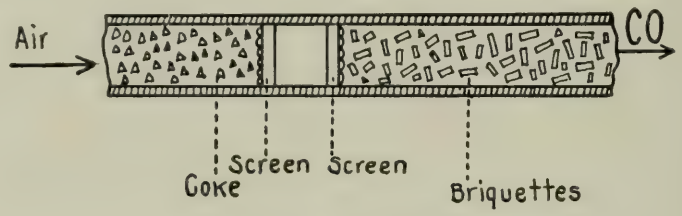

FIG. 11.

the use of air directly, providing it passed through hot coke before it reached the catalytic solution surface.

A number of runs were, hence, made in horizontal iron pipes of 1 inch diameter as shown in Figure 11.

Several inches of layers of coke were held in position in the tube with iron screens and then another screen kept the briquettes away from the porous absorbing surface of the coke. The pipe was heated to the cyanizing temperature and a current of air then passed in. Of course, this gave an extra fine carbon monoxide flame. When the process was over the air was shut off before the tube was cooled.

Similar runs were made in the apparatus by using pure nitrogen and the results were just the same as when air was used. In other cases the producer gas was made in a separate furnace and then passed into the cyanizing tube.

There was no apparent difference in any of these results. It is, hence, evident that the process is independent of pure nitrogen

and that air, producer gas, flue gas, or even the gases from the combustion chamber of the furnace, could be drawn through the cyanizing tube just as well. To make the test still more striking on a commercial scale, two pipes of iron 8 feet long were put side by side vertically in a furnace as shown diagrammatically in Figure 12.

One pipe was 6 inches in diameter and as I had no other of

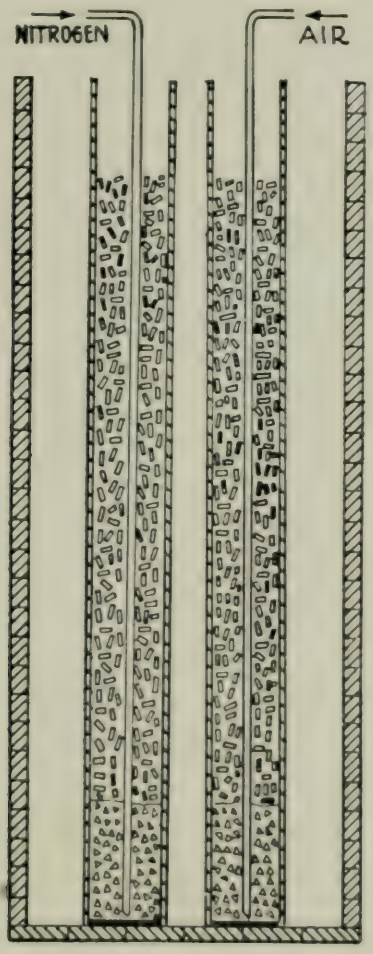

FIG. 12.

this size, the other was taken of 4-inch diameter. The pipes were closed with caps at the bottom and then iron pipes of ½-inch diameter were thrust to the bottom of the larger pipes and about a foot layer of coke was dropped into each one. Then 72 pounds of briquettes were poured into the larger pipe and an equally deep layer was placed in the 4-inch pipe.

The whole apparatus was now heated red-hot for a considerable

time and pure nitrogen passed into one and air into the other. The result was 19% of NaCN in the pipe which had the air and 18% in that through which the pure nitrogen passed. These results are practically identical, thus showing that air can be used and that the process can now be operated anywhere. You can now see how easily producer gas could replace the nitrogen in working the electric furnace shown above in Figure 8.

PRODUCTION OF NITROGEN.

Although I do not need nitrogen in my fixation process with carbonates, it is, nevertheless, very convenient to have it for test and when working with alkali metals themselves. The passing of air over heated copper has many great advantages, particularly in the fact that the reagent is solid. Notwithstanding this, there seems to be much prejudice against it commercially and it was condemned as impractical or impossible just as many of the other basic things I have described to-night.

As no one cited any physical or chemical principle against it which seemed reasonable, it was in order either to show that it could be done or else why it could not be done.

Accordingly a 2-inch iron tube 40 inches long was filled with the turnings which result as a waste product when the copper rolls used in the textile industry are turned down in a lathe. These turnings can be gathered in quantity and at a price below that of copper ingots. The copper used may contain considerable zinc and still be entirely suitable for the work. Four pounds were tamped into the iron tube to give a 28-inch column.

These tubes were heated in a combustion furnace to a low red heat and they easily gave 200 liters of nitrogen before they were exhausted. Then they were reduced with hydrogen and were again ready. I never used gasometers, but simply passed the air blast furnished by a small glass suction pump over the hot copper and then used the gas directly in the runs.

This shows how easily one can get the current of nitrogen automatically at constant pressure in any quantity varying from a few bubbles to two liters per minute. The tubes were run for months and were just as good as when they were first made.

It was then desired to construct a large commercial unit and in order to test how much sagging, analogous to that shown by

the briquettes in Figure 6 would take place, the horizontal tube was hence purposely taken 12 inches in diameter by ten feet long and charged with the copper turnings. It gave 300 liters of nitrogen per minute at first but in a few days was completely useless. Examination showed that the copper had sagged 4 inches and was, moreover, covered with iron scale from the tube so that the air did not get in contact at all. In another case, about a 6-inch tube was placed vertically but the plastic copper soon compacted by its own weight so as to shut off the gas current.

These factors, having been determined to be large, it was evident that they must be avoided. This was very easily done and the first furnace constructed did so well that I should simply duplicate it if more nitrogen were needed.

I used a vertical pipe 10 inches in diameter and 6 feet long, which was heated in a furnace having two vertical flues, one for the burner flame and the other for the tube containing the copper. The supports for the copper were made from two ordinary 1-inch iron crosses with side openings which were connected by a threaded 6-inch piece of iron rod through the side openings. Then eight 4-inch iron nipples were turned into the eight remaining openings so as to make the eight-armed support so that it would just slide down into the 10-inch pipe.

One was dropped in and the copper tamped down just level with the top of the cross with a stick. Then another support was dropped down so that it rested on top of the lower cross and copper was tamped in as before. This was continued until the top was reached and the pipe contained 280 pounds of copper turnings.

You will see that every section of copper was thus supported firmly and independently. It could not sag from the sides because the tube was vertical. It could not compact itself because each section only had to sustain its own weight and whatever sag there was in a section could simply open a horizontal section, which would do no harm.

This furnace was run hard for six weeks in the tests and I could not detect the slightest deterioration. It gave 16,000 liters of nitrogen in two or three hours before requiring reduction and then it was reduced by cautiously adding a number of liters of wood alcohol at the top. This served not only for reduction but also as a measure of the capacity of the furnace. I never heated it above 450°C. In actual practice one would reduce the copper

with the carbon monoxide from the cyanizing furnace and on a large scale perhaps it might not be necessary to apply external heat since the reactions in the copper turnings are exothermic. The reductions should hence not be too energetic as otherwise it might heat the copper too high. I do not know how others construct their furnaces but this one is eminently satisfactory.

The expense of copper need hardly be considered because the cyanide produced would now pay for the copper every forty minutes and even in normal times in less than half a day. If any one is doing electrolytic work and has waste hydrogen, this will also give nitrogen with the utmost ease since it is only necessary to burn the hydrogen and air in regulated streams in a very simple apparatus.

It consists simply of iron tubes which may be lined with fire brick inside in the combustion space.

This is about the same as the above scheme with copper, only the copper is left out and the hydrogen and air act simultaneously. The steam formed condenses out and leaves the nitrogen which may contain a slight excess of hydrogen if desired and the heat generated can be used for evaporation purposes.

A still simpler device follows from my method of changing cyanide into urea, as shown below. In fact, it will be seen that the nitrogen there becomes a waste product in nitrogen fixation and it may accumulate twice as fast as it is being fixed.

DISTILLATION OF CYANIDE.

Having now shown methods of obtaining alkali cyanides, it is naturally in order to consider methods of getting the cyanide from the briquettes. One would wish to avoid lixiviation if possible, and, hence, the following work on distillation. As we would destroy the catalytic solution surface by heating the iron-carbon mixture above the eutectic point (about 1120°C.) because of the iron melting to globules, it seemed worth while to try the distillation in either a current of nitrogen or in a vacuum. Both seem to work.

For example: Some cyanized briquettes were placed in a copper tube, heated to 1000°C. and at about 2 mm. pressure. The cyanide collected in a pool which solidified to a clear mass which could not be told from glass by appearance. There was no cyanide

whatever left in the iron-coke mixture and only the merest trace of alkali.

In another case the specimen of distilled cyanide was titrated and found to be 99.9% NaCN. I have also distilled both sodium and potassium cyanides in vacuo while a melting point tube filled with sodium chloride was in the mixture. The sodium chloride did not melt, hence the cyanide must have distilled below its melting point (792° or 820°C.).

$K_3Fe(CN)_6$ and $K_4Fe(CN)_6$ were also heated in a vacuo. This gave a fine distillate of KCN while iron-carbon in very finely divided form remained. This would be slightly wasteful but would work finely since the iron, carbon and nitrogen obtained in decomposition would go back into the cyanizing process again.

ASHLESS CARBON.

The great promise of the above method of distilling cyanides would soon come to grief by having the ash from the coke accumulate with each repetition until it would stop the process. If we could get this carbon from carbon monoxide, it would be ideal. This suggests the following equation:

$$2CO \rightleftarrows C + CO_2 + 38{,}080 \text{ calories.}$$

The equilibrium for this equation gives almost pure carbon monoxide at 1050° C. while at 500° C. it is almost reversed, so that carbon monoxide must be unstable at low temperatures and if we could find a suitable catalyzer we should be able to get carbon from it.

I first passed pure carbon monoxide over ground coke but could notice no change. No carbon dioxide could be detected with the Hempel burette and only a trace of precipitate was obtained with lime water. Evidently the catalysis was negligible.

The experiment was repeated as before in a combustion tube of hard glass which has a layer of finely divided iron in the bottom. A very striking change took place at once. The surface of the iron blackened at once and the issuing gas contained over 40% of carbon dioxide in spite of the rapid current and the very slight contact. The catalysis must, hence, be exceedingly efficient and in a few hours the bulk of the whole charge had so increased

from the deposited carbon that the entire section of the tube became filled so that the gas could no longer pass through.

The wonderful importance of this when applied to the iron-coke-soda ash can be seen at once. It gives us ashless carbon, in the form of very finely divided lampblack, together with 38,080 calories of heat developed within the charge itself. In addition, since the coke does not appreciably catalyze the reaction, the deposited carbon must hence be in very close contact with the iron.

We thus have this automatic process to do away with the ash in the carbon, with grinding the carbon, with mixing the carbon and iron, and it provides internal heat in the briquettes themselves. It is, hence, equivalent to that much internal electric heating.

I will show how this will apply later under the theory of the continuous furnace. Suspecting that Mond[19] might have done such work in connection with his metal carbonyl compounds I looked up the literature and found that he had done so. He found that with 15 parts of nickel, he could deposit 85 parts of carbon.

I also noticed that I could not obtain over about 43 to 45% of carbon dioxide in the experiments and this suggests that there must be an equilibrium which should not be here if the process were one of pure catalysis since the catalyst should simply hasten the velocity of transformation but should not completely change its course.

A search of the literature revealed considerable work along this line from the point of view of the phase rule. Findlay[20] discusses the work of Baur and Glaessner, Boudouard and Hahn.

Figure 13 shows a diagram embodying part of their important work on the equilibrium relation of carbon dioxide, carbon monoxide, iron and carbon at varying temperatures, while the dotted line in the curve shows what the relation should be if no catalyzer were present or if the catalyzer exerted no foreign function.

It is evident at a glance that as soon as the concentration of CO_2 reaches a certain value, it will begin to oxidize the iron to FeO, thus destroying the catalyzer and completely changing the action.

The curve ACB, of the Figure 13, marks the concentrations of CO and CO_2 which correspond to each temperature as long as any metallic iron is present. The maximum value we can get for

[19] Jour. Chem. Soc., 57 (1890), 749.

[20] "The Phase Rule," Third Impression, p. 306.

the formation of CO_2 is, hence, about 42% at the temperature of 680°C.

On the other hand, nickel does not form oxides as readily as iron and it will hence transform CO almost quantitatively into CO_2. This can be shown beautifully as a lecture experiment by placing finely divided nickel in a glass tube heated to perhaps

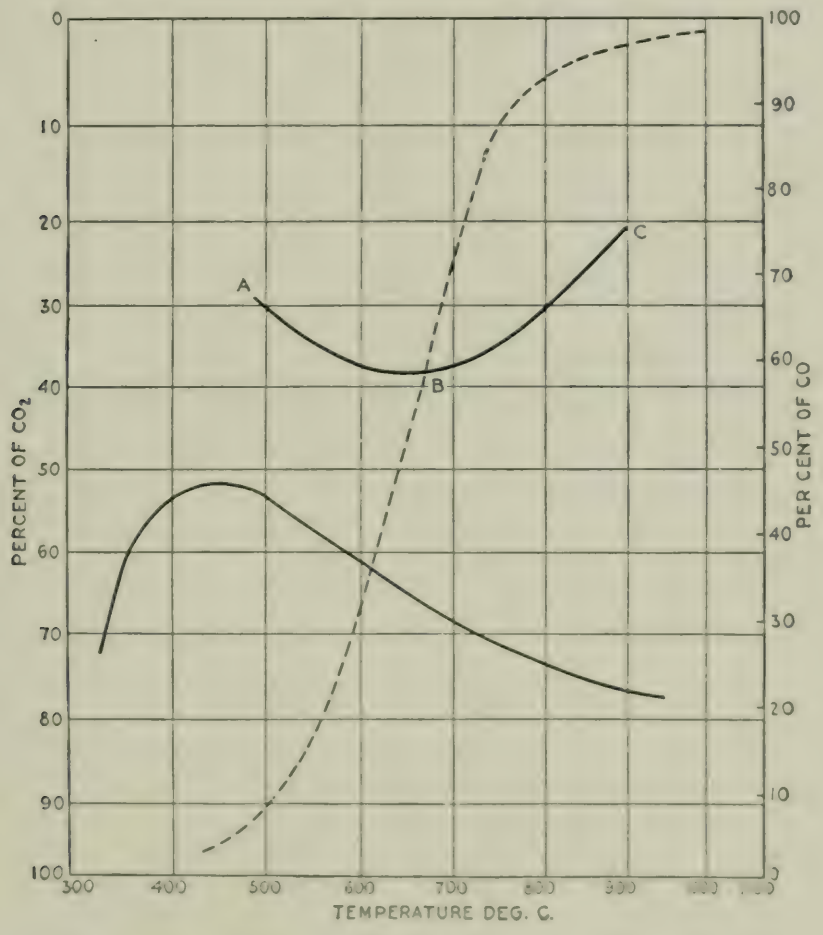

FIG. 13.

300° to 400°C. When pure CO is passed into the tube it is so completely transformed that the CO flame goes out and the exit gas contains 98% of CO_2.

These phase-rule studies are of great importance in the study of blast-furnace reactions, casehardening, etc., and I will further apply them under the question of the oxidation of iron pipes.

OXIDATION OF IRON PIPES.

In conducting the above process by external heating, the question of oxidation of iron pipes must be seriously considered, but from some thousands of experiments I feel sure that this will not be of the slightest trouble when operations are based upon scientific principles. If we use electrical heating, the oxidation is absolutely avoided in the furnace shown in Figure 8, which was designed and built for this very purpose.

My experience leads me to believe that it can be entirely avoided in fuel-heated furnaces. For example, the temperature-concentration diagram in Figure 13 shows that iron oxide can not exist in the field above the curve ABC and, hence, it is impossible even to oxidize the iron pipe until the heating flame contains the corresponding value of CO_2.

For instance, if the iron pipe is heated to 950°, then we could have 25% of CO_2 present and still have oxidation impossible. That is we could burn carbon according to the equation:

$$4C+2\tfrac{1}{2}O_2=3CO+CO_2+\ 184{,}830 \text{ calories}$$

without any possibility of oxidizing the tube and then the remaining 203,010 calories could be obtained by admitting enough air in the preheating zone to burn the 3CO where oxidation would no longer take place because of the lower temperature. We might also combine the principle of the regenerative oven here if desired.

These figures would be modified somewhat by considering the diluting influence of nitrogen, but I have only time to state general principles without giving details. It is very likely indeed that much more heat may be liberated in the heat zone because of the protective influence of a layer of oxide on the surface of the tube, but I always prefer to calculate on the most unfavorable case.

Also, we might consider admitting hydrogen with the producer gas as by having a steam-blown producer. The hydrogen present would then steadily diffuse through the red-hot iron walls, as explained above under Figure 1. The outward diffusing hydrogen, aided by the outward diffusing carbon and carbon monoxide, would meet the ingoing layer of oxide and would drive it back under the right conditions.

Also copper tubes would absolutely avoid this oxidation and

they would easily stand the temperature. I have, in fact, used copper tubes largely at 950° to 1000°C. in another line of work with the greatest success. I could notice no damage to the tube either inside or outside. The expense of the copper would not be prohibitive and it could be cut down by only using it in the actual heat zone, and by using it as a sheathing for iron tubes and perhaps iron plated with copper would do. I have also tried nickel sheathing with results which would warrant a longer test.

There is no doubt that the trouble from the oxidation of iron tubes has been grossly exaggerated, and, personally, I feel convinced that there is nothing to it.

THEORY OF BATCH FURNACE.

The theory of the batch furnace is also the theory of the process in its simplest form. We simply grind together intimately, iron, coke, and soda ash, moisten and briquette the material. The dry briquettes are then heated in a vertical pipe to say 950°C. in a current of nitrogen or producer gas until carbon monoxide no longer escapes. This is exceedingly simple as is all that there is to the process.

The theory is that the iron, by dissolving the carbon, gives a solid solution in which the carbon may move freely and it becomes active. This gives us our "catalytic solution surface."

It is hence only necessary to produce the extremely active catalytic solution surface to maintain it unimpaired during the process, and to give the nitrogen and alkali metal or compound free access to the surface during the run.

ENRICHING PRODUCER GAS.

You will note from the equation that nitrogen gas is absorbed in the process and carbon monoxide is formed in large volume. The gas is enriched in a twofold way and I have noted 75% of carbon monoxide in the exit gas from the batch furnaces.

Bunsen and Playfair absorbed all their nitrogen; hence it is probable that under the right conditions producer gas can be enriched so that it will be substantially pure carbon monoxide.

DESTRUCTIVE EFFECT OF CARBON MONOXIDE.

You will notice that in using producer gas, I always shut off the gas current before the briquettes began to cool. This is a vital point. For example, briquettes containing 19% of sodium cyanide were heated red-hot in a current of producer gas and cooled slowly while the current continued. When cold, they contained only 3% of cyanide of sodium. This shows that we have almost complete destruction of cyanide when the briquettes cool in a producer gas current and that we avoid all trouble in the batch furnace, Figure 8, by merely stopping the current before the charge cools or by dumping the hot briquettes so that the gas current can not pass through while they are cooling.

This destruction may well be due to carbon dioxide formed by the catalysis of the iron in causing the formation of ashless carbon by the equation:

$$2CO \rightleftarrows C + CO_2.$$

If this be the true explanation, the iron acts both as a destructive and constructive catalyzer in the process.

If this be so, then at 500° to 600°C., cyanide should be destroyed rapidly (see the temperature concentration diagram, Figure 13) while, at, say, 950° to 1000° C. the CO_2 has almost disappeared and the speed of the cyanide formation has increased so that no harm is done. This is very important in the continuous furnace shown in Figure 14.

THEORY OF THE CONTINUOUS FURNACE.

It would take a separate paper to deal with the general features of such a furnace as that shown in Figure 5. The briquettes passing in the top become preheated by utilizing the waste heat from the gases of the reaction zone, and then collect all the cyanide fumes which are liable to distill in clouds from the batch furnace. When they get red-hot they will, by catalysis of their iron content, produce the reaction:

$$2CO \rightleftarrows C + CO_2 + 39\,960 \text{ calories.}$$

This gives finely divided ash-free carbon which is necessarily in intimate mixture with the finely divided iron. It also gives out heat in the briquettes themselves and therefore has the same exceedingly important effect as internal electric heating. Unfor-

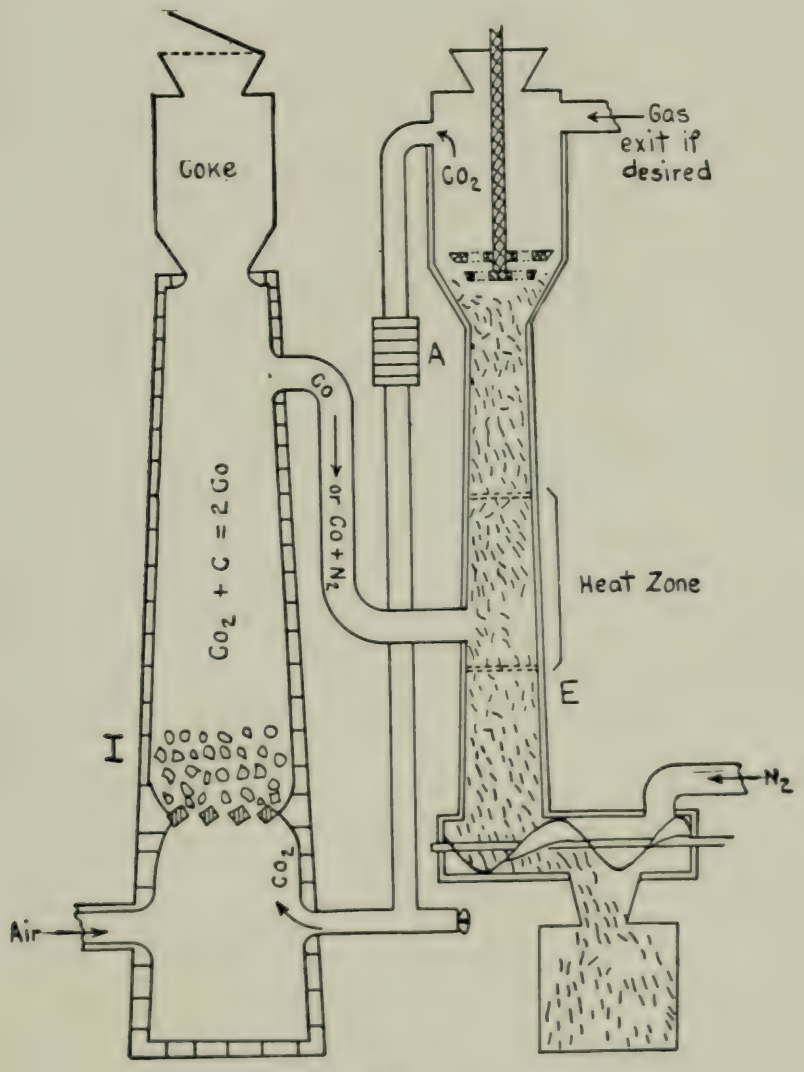

FIG. 14.

tunately the equilibrium relations of curve ABC in Figure 13 must be applied, but, even if we take the maximum value as 680° C. this would contribute over 20,000 calories towards the 138,500 calories needed in the heat zone for the reaction.

$$Na_2CO_3 + 4C + N_2 = 2NaCN + 3CO - 138{,}500 \text{ calories.}$$

This is an exceedingly great advantage. Then in the cyanizing zone the same reaction takes place as in a batch furnace, but the intense volatilization from the porous briquettes carries the alkali upwards to be recondensed on the cooler briquettes above with corresponding heat changes. This distillation gives fine circulation of alkali to the solution surface and is carried back by the descending briquette column.

Below the cyanizing zone, the nitrogen cools the briquettes and itself becomes preheated so as to carry the waste heat back into the heat zone. The plastic briquettes now harden again and are removed by the conveyor. When producer gas washed with caustic alkali to remove CO_2 is used instead of nitrogen we have in some ways a very different problem.

For example, in a run in a 14-foot, 6-inch pipe which should have yielded at least 20% of NaCN in the briquettes, I got only 5% of cyanide when feeding at the rate of 2000 lbs. per day. When the feeding was reduced to 1000 lbs. per day the yield dropped to 1% of NaCN in the briquettes. In view of the facts already disclosed it is evident that the producer gas destroyed ¾ of the cyanide formed with 2000-lb. feed, and when this was made slower the producer gas had a longer time to act and 19/20 of the cyanide was destroyed.

This looked bad for the continuous process with the producer gas. The difficulty was overcome, however, by simply passing the producer gas into the briquette column at the base of the heat zone. In this case the cyanized briquettes as they cool are entirely away from the current of producer gas (there must be no leak in the lower fitting of the furnace pipe, otherwise some of the producer gas would be forced downward with the briquettes and thus do harm).

This arrangement is shown diagrammatically in Figure 14 and is arranged so that we may use either nitrogen or producer gas and it shows the principle of transferring carbon from one part of a closed system to another with oxides of carbon so as to separate from ash; incidentally, such simple modification in the furnace gives 25% NaCN in the briquettes.

Even poor briquettes will give good results in these continuous furnaces. Some of the advantages can, however, be attained in batch furnaces by running the waste gases and fumes from the top of one into the bottom of the next furnace.

LIMITING FACTS.

Suppose we consider for example a vertical 6-inch iron pipe, 6 ft. in length, charged with briquettes of the usual size. Thus the speed of operation would be limited by the activity of the briquettes, by the rate at which nitrogen could be passed in, and by the rate at which heat could penetrate through the charge to the center of the cone. Results already mentioned show that the briquettes are active enough to be cyanized in one or two minutes. The requisite nitrogen could be passed through them in ten minutes. The heating could be done in two hours without undue excess of temperature outside.

These data show why heat conduction is the slow factor and that internal heating by electricity or by the ashless carbon method are so interesting. Also the slow conduction determines the furnace design. It means above all else a long heat zone, just as long as possible, so that it will not be necessary to force an excessive amount of heat through a short heat zone with all the ruinous effects that follow.

I should use the full length commercial pipes of 20 to 24 feet length unless the plastic briquettes compacted too much. In that case I should try slanting the pipes and if this did not work I would cut the pipe so as to bring the preheating zone in at an angle which should be sure to relieve the pressure.

SODIUM FERROCYANIDE.

Having now shown how to fix nitrogen in the briquettes, it is in order to show how compounds can be recovered from them. If we add hot water to the cyanized briquettes to make a stiff paste and then steam them in an agitator you will see at once that we have an electrolyte of sodium cyanide and innumerable carbon-iron elements in contact with it.

We might hence expect to get as a result of electrical action, or otherwise, the following reaction:

$$6NaCN+Fe+2H_2O=Na_4Fe(CN)_6+2NaOH+H_2.$$

This reaction takes place with great ease, hydrogen being given off in large quantity. After agitating for several hours, the hot

mass is filtered and the black powder turned back into the process again.

The hot filtrate deposits an exceptionally fine quality of sodium ferrocyanide on cooling, and upon concentrating the filtrate the rest deposits out from the strong solution of caustic soda which is formed. The caustic soda is either turned right back into the process or used otherwise. The method is exceedingly effective.

LIXIVIATION FOR CYANIDES.

From this behavior of the briquettes with hot water, we see that it will be necessary to observe certain precautions in lixiviat-

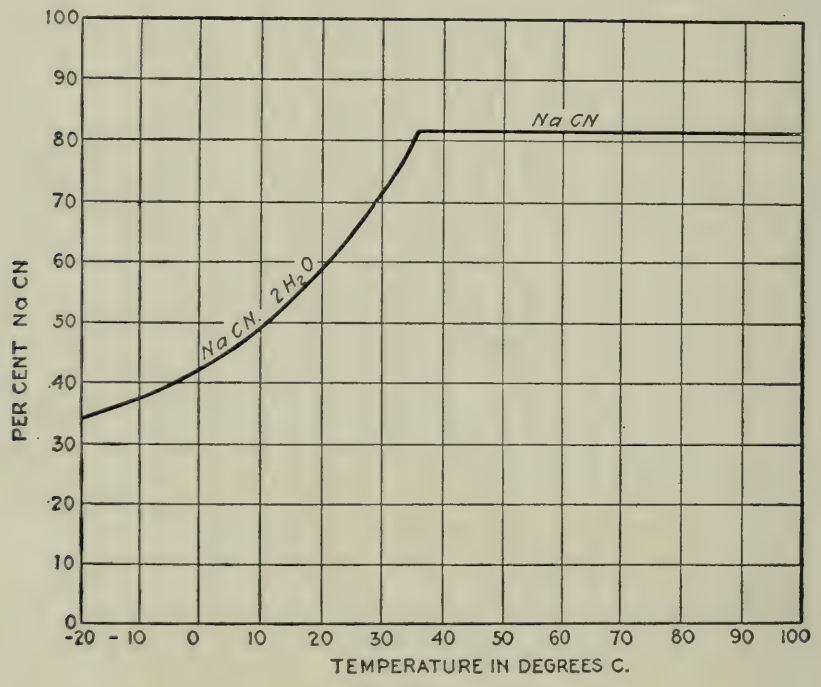

FIG. 15.

ing to get sodium cyanide. On the other hand, the solubility curve shown in the Figure 15 shows that at temperatures below 35° C. the mass will take up water of crystallization to form $NaCN.2H_2O$ which will "set" the mass just like plaster of Paris. To avoid both dilemmas we should lixiviate at a temperature slightly above 35° C., the transition point of the cyanide. This should be done quickly because the ferrocyanide reaction is going on slowly even at this temperature.

The solution can be concentrated in vacuo if desired. The

cyanide is pure enough for many purposes. Or, it can be distilled as mentioned above. I have sometimes added lime to it to causticize any soda ash present as this would prevent the hydrolytic dissociation from hydrocyanic acid:

$$NaCN+H_2O \rightleftarrows NaOH+HCN$$

for which manufacturers frequently add caustic soda intentionally.

Also we may add calcium chloride to remove the carbonate and replace it by sodium chloride thus:

$$Na_2CO_3+CaCl_2=CaCO_3+2NaCl.$$

On then evaporating, we have the mixture of sodium chloride and sodium cyanide which is so much sought in the jewelry trade.

AMMONIA AND SODIUM FORMATE.

If we boil sodium cyanide solution to get ammonia and sodium formulate,

$$NaCN+H_2O=HCO_2Na+NH_3,$$

we can only get to the boiling-point of the solution. The action is slow and hydrocyanic acid is likely to form according to the already mentioned hydrolytic dissociation.

If, however, we add caustic soda (or prepare it with lime in the lixiviated solution) we get these effects: the caustic soda prevents hydrolytic dissociation to form hydrocyanic acid, it raises the boiling-point of the solution and it salts out the sodium formate from the solution by making it many times less soluble than in pure water.

The operation is carried out with a return condenser and the ammonia after passing through a caustic soda dryer is chemically pure and anhydrous and can be compressed at once into cylinders, if desired. The sodium formate can be turned back into the cyanizing process or it can be converted into sodium oxalate at once by simply heating in vacuo, and from these we can easily obtain formic and oxalic acids.

UREA, AMMONIA AND NITROGEN.

If we arrange a flanged cast-iron pot, 3 inches in diameter by 12 inches deep, with a cover, inlet and exit pipes, and a 2-inch reducing coupling as shown in Figure 16,

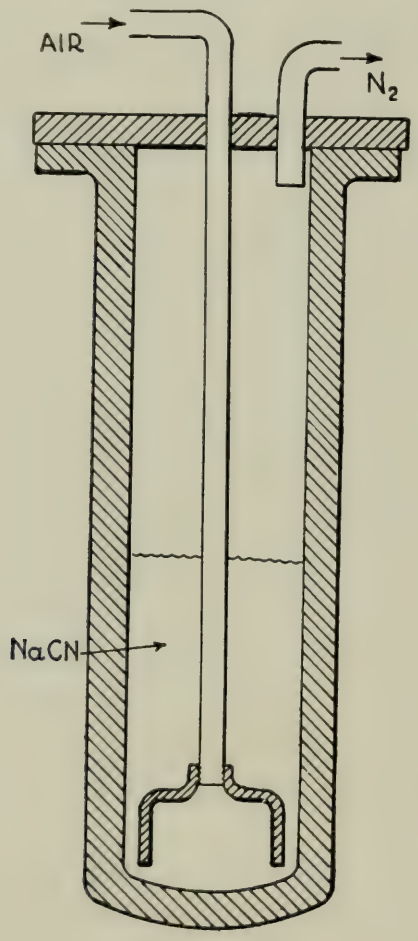

FIG. 16.

we have a serviceable laboratory unit to convert cyanide into urea. The cyanide is kept just above its melting point and a current of air is passed through the molten cyanide. It oxidizes to sodium cyanate according to the approximate equation:

$$NaCN+air=NaCNO+2N_2.$$

The nitrogen thus formed is turned into the cyanizing furnaces and this gives us four atoms of nitrogen for each two atoms fixed.

The process hence takes its own nitrogen from the air to such an extent that theoretically one-half would have to be thrown away as a waste product in spite of the process being one for the fixation of nitrogen.

One-half of the cyanate is now heated with water which converts it into ammonia long before the boiling point is reached according to the equation

$$NaCNO+2H_2O=NaHCO_3+NH_3.$$

The sodium bicarbonate is thrown back into the process and the ammonia saved or else converted into urea as follows:

The other half of the NaCNO is added to water and the ammonia then passed in. Then CO_2 from the cyanizing furnaces is also passed into the liquid. The change is represented:

$$NaCNO+NH_3+H_2O+CO_2=NaHCO_3+NH_4CNO$$

and

$$NH_4CNO=Urea=CO(NH_2)_2.$$

You will notice that this is exactly like the ammonia-soda process:

$$NaCl+NH_3+H_2O+CO_2\rightleftarrows NaHCO_3+NH_4Cl;$$

only the radical (CNO) takes the place of the chlorine atom of sodium chloride.

The urea reaction takes place much better than the ammonia-soda one because the NH_4CNO passes into urea and hence practically does away with reversibility while the NH_4Cl in the ammonia-soda process cause a loss of 30 to 40% because of this factor.

The bicarbonate is returned to the process. We thus have a very inexpensive way of getting urea which has over 46% of nitrogen and which is about three times as rich in nitrogen as sodium nitrate. It is about twice as rich in nitrogen as ammonium sulphate and would not introduce sulphuric acid into our soils, many of which are already too acid.

The urea was tested in water culture in the botanical laboratory of this University and found to give results equal to that of the nitrogen from potassium nitrate.

The urea may also be combined with nitric acid produced from ammonia to form urea nitrate $CO(NH_2)_2.HNO_3$ which is still very rich in nitrogen but less soluble than urea.

METALLIC SODIUM.

Sodium cyanide melts at a much lower temperature than salt and requires much less voltage for electrolysis. My experiments indicate that it can be easily electrolyzed

$$[2NaCN = 2Na + (CN)_2]$$

to sodium and cyanogen.

This should give us a very cheap way of getting sodium from sodium carbonate with the incidental fixation of nitrogen.

OXAMID, OXALIC AND FORMIC ACIDS.

The cyanogen obtained from above was not supposed to be readily convertible into oxamid, but I find it converts with the utmost ease under the right conditions. The gas is rapidly absorbed in hydrochloric acid (44%) and then changes to oxamid thus:

$$(CN)_2 + 2H_2O = (CONH_2)_2,$$

which being practically insoluble separates out as a pure white powder.

Here we have the remarkable case of the hydrochloric acid acting as a catalyzer to concentrate itself even to the point of becoming gaseous, while at the same time precipitating out the resultant compound.

Curiously enough, the reaction practically stops when the hydrochloric acid is slightly dilute (14%). The oxamid has nearly 32% of nitrogen and is nearly insoluble in water and it should be especially of value as a fertilizer and should partially approximate the effect of the nitrogen in dried blood rather than that from the more soluble sodium nitrate. This has apparently already been verified by Dr. Hartwell of the Rhode Island Agricultural Experiment Station.

We get chemically pure oxalic acid from the oxamid by simply

heating it for a minute or so with the concentrated hydrochloric acid to add more water, thus:

$$(CONH_2)_2+4H_2O+2HCl=(COOH)_2.2H_2O+2NH_4Cl.$$

The acid crystallizes out free from all mineral salts.

The acid upon heating with glycerin transforms at once into formic acid according to the equation:

$$(COOH)_2=HCO_2H+CO_2.$$

CAUSES OF FAILURE.

A few of the large number of reasons for the absolute failure to achieve commercial results worthy of consideration by the methods described in this paper may be given.

The omission of iron (which, by the way, is probably the only technically suitable catalyzer where alkali carbonates are used) absolutely ruins the process. Failure to properly mix the constituents is nearly equally ruinous and has no doubt contributed largely to failure. Heating a briquette a little too high will melt the iron at the eutectic point into globules, thus destroying the extended catalytic solution surface and thereby completely ruining the process. It is also above shown that simply putting a large vertical retort in the furnace horizontally will completely ruin the process whenever the charge is plastic.

Alder preferred to mix his powdered materials with water and then with pieces of charcoal from "size of pea to size of fist." You can see at once that here the charcoal would absorb the molten cyanide just as a sponge absorbs water, thereby preventing the alkali from contacting freely with the catalytic solution surface even when he had one present by using iron. To show how serious this is, I prepared an active iron-coke-soda ash mixture according to my specifications which yielded cyanide of 98% purity. A portion was then mixed with water and charcoal and it now yielded cyanide of only 14% purity. This shows conclusively, that Alder's preferred directions will ruin even an initially active mixture.

You can now see why I was careful to have the screens spaced in Figure 11 to keep the coke and briquettes apart. The above work shows that cooling a cyanized charge containing iron in producer gas ruins the process. Evidently oxygen would be worse

and you can form your own inference about cooling the descending charge in nitrogen containing 2.5% of oxygen.

The quantitative measurement of heat penetration also shows that the heat zone should be long so as to allow time for the heat to penetrate to the center of the charge without having to force the process and thereby causing no end of needless trouble.

EMERGENCY PROCESS.

I have carefully distinguished between what I have actually demonstrated with units which were of commercial size and those other things which have not been worked out as far.

In view of the conditions now existing, I wish to call attention to this topic so definitely that the issue will be so sharply defined that any one interested can test the matter for himself by direct experiment within a few hours. This is a perfectly fair way of finding the truth or falsity of the statements and criticisms I have felt compelled to make.

If one wishes an operative process at once, it is only necessary to construct the furnace shown in Figure 8. Construct a platform about 6 feet by 8 feet with 6 feet of free space underneath out of 4 by 6 timbers and 2-inch planks, cutting a hole through which the iron pipe may pass. Put on four small piles of brick to support the metal drum, which should be, say, 30 inches in diameter by 6 feet in height, and fill it with magnesia asbestos after inserting the drilled pipe and fastening with locknuts. Put on the reducing coupling, stuffing box and iron support, as shown in Figure 8, and drop in the brick support 8a and turn on the eight-inch receiving pipe.

This completes the furnace and with the help of several workmen I would find no trouble in doing the whole thing in six hours. Briquettes are now added and the copper leads attached. With sufficient current the first run will be finished and drawn within two hours and then runs can be made every hour; this can be kept up indefinitely if the drum be airtight.

This furnace can be in operation in less than 24 hours, giving its output steadily and will easily yield 150 pounds of sodium cyanide per 24 hours or over 200 pounds of sodium ferrocyanide in the same time. An ordinary meat chopper will easily supply briquettes enough to run eight such furnaces.

With over $2.00 per pound now asked for sodium cyanide, and with sodium ferrocyanide at over $1.25 per pound and potassium ferrocyanide at over $7.00 per pound within the past 10 months, and great anxiety in both the mining and jewelry industries, it seems that the above furnace which costs less than $100.00 and can be put up and operating in less than a day might have been tried.

It would, at a stroke, either have shown how to relieve the situation immediately or else have shown that what I have been telling you to-night is worthless.

If no high amperage is available, the ordinary lighting current of 120 or 220 volts with iron rods analogous to the construction indicated in Figure 9 would work just as well. If electricity were not available then an oil burner would answer just as well, as any burning-out of pipes would not need to be considered at such prices. I am not here advocating any particular furnace, hence there is no cause for discussion about its merit or lack of merit. It simply shows a means of getting away from endless discussion by doing something to get chemicals which are sorely needed.

These units could be enlarged and put in series to take practically any current, and I have data for the design of a plant capable of producing the ammonia for 180,000 tons of nitric acid per year along these lines and it would be something which could be done quickly in case of need.

COST OF PROCESS.

I do not have time to go into the question of cost of production this evening, but I have estimated it very carefully from what I have actually tested with commercial units as well as from theory. I will hence say that the equation

$$Na_2CO_3+4C+N_2=2NaCN+3CO-138,500 \text{ calories}$$

would theoretically require about 35,000 H.P. to produce 180,000 tons of nitric acid, allowing 85% efficiency in oxidizing the ammonia.

But the three molecules of CO in the above equation would give on burning

$$3CO+1\tfrac{1}{2}O_2=3CO_2+200,000 \text{ calories.}$$

equivalent to about 50,000 H.P. to help make up the unavoidable heat losses. Combining these two equations we have

$$Na_2CO_3 + 4C + 1\tfrac{1}{2}O_2 + N_2 = 2NaCN + 3CO_2 + 61{,}500 \text{ calories.}$$

The total process is hence really an exothermic one and, in any case, it does not require electric power. Owing to its great simplicity and to the inexpensive materials, the cost is exceptionally favorable for the fixation of nitrogen and the production of cyanides, ammonia, urea and the other substances above mentioned.

COMPARISONS.

We are now in a position to compare the above process with three of the commercial methods now in successful operation. The arc process which has been developed on such a large scale in Norway yields nitric acid and nitrates but it requires a very high temperature and is hence not only absolutely dependent upon electric power, but I think all agree that it can succeed only where the power is very cheap because the requirement of current is very large. It is further handicapped by yielding very dilute products which must be worked over.

The cyanamid process requires calcium carbide which can practically be made only at the high temperatures of the electric arc. It is therefore also absolutely dependent upon electric power but it can make use of more expensive power than the arc process. It has hence a correspondingly wider choice of locations. It is also handicapped by using nitrogen itself rather than producer gas.

My process differs from the above processes by operating at temperatures which may be below 950° C., and it is therefore independent of electric power. This means that it can be operated at any locality suitable for manufacturing purposes. I can use producer gas just as well as nitrogen, and the process is therefore independent of pure atmospheric nitrogen. It is so exceedingly simple that it can be installed immediately with apparatus obtainable in the open market, and because of the remarkable transformations of the cyanogen it opens the way for the cheap production of organic substances such as urea, oxamid, etc., as well as new commercial methods for oxalic and formic acids and many other substances I have in mind.

The Haber process fixes nitrogen by combining it with hydrogen and requires these substances in the pure state. It requires costly machinery, and its development required the highest type of engineering skill. These highly specialized things, however, mean that it will also require especially competent people to run it.

On the other hand, the process I have described to you this evening represents the other extreme. It makes use of the crudest things such as coke, producer gas, and water. These are handled by the simplest possible mechanical operations and the catalyzer, iron, is not bothered by the gross assortment of elements in the ash of the coke, and any sort of labor, when properly directed, can handle it.

REMARKS.

It is not worth while to make a summary of the work I have described, because the whole presentation has been condensed so as practically to be a summary. Almost everything mentioned can be done in a variety of ways which I did not have time to mention, and even the most important things could not be much more than mentioned.

I hope that the evidence presented has cleared away some of the erroneous statements that have hindered progress in this field which has been handled in an exceptionally unfortunate way. I also hope that the statements under "Emergency Process" will, if sudden need arise, lead to instant action instead of interminable discussion and counter-propositions. The way to test the matter is to set up the apparatus as is sharply set forth. A roof can be put over it after it is in operation and blueprints, plans and improvements can await their turn.

None of the things mentioned in the above work were discovered by chance. All were predicted by a carefully considered application of the very simple but fundamental principles of physics, chemistry and mathematics which every undergraduate in chemistry is taught. They were then verified by experiments which were chosen to give decisive answers to questions and to establish quantitative data.

Only a comparatively short time could be given to the work because of the heavy pressure of other duties, and it would certainly have been impossible to carry it out if it were not true that pure science is the only foundation for industrial work.

I have been connected with universities and technical schools for 28 years as student and teacher. I have always used every effort to further the view that all industries must be based directly on work in pure science, and that the only way to succeed was to become thoroughly master of these fundamental principles and then look at things from every point of view in order to solve whatever problems may be encountered.

There seems to be a constantly increasing pressure both from within as well as from without the University which is inimical to this ideal, and many cures, which are at variance with it, are advocated, in the rush to get results quickly by a short cut to knowledge.

If it had been more generally realized that hard work and careful, self-sacrificing preparation are very likely ultimately to lead to quick, certain solution of difficulties rather than perhaps to years of misguided effort and enormous expenditures of money, with, like as not, ultimate failure, things would not have drifted to their present pass.

If these illustrations have some influence in this direction I believe that it will be more important to the country than even a successful nitrogen fixation process.

BROWN UNIVERSITY,
Providence, R. I.

CONSTITUTION

ARTICLE I.

NAME.

This organization shall be termed,

AMERICAN INSTITUTE OF CHEMICAL ENGINEERS

ARTICLE II.

OBJECTS.

The objects of this organization shall be:

To advance the cause of applied chemical science.

To give the profession of Chemical Engineers such standing before the community as will justify its recognition by Municipal, State, and National authorities in public works.

To raise the professional standard among Chemical Engineers, discouraging and prohibiting unprofessional conduct.

To coöperate with educational institutions for the improvement of the education of the men who are to enter this profession.

To encourage original work in chemical technology.

To promote pleasant acquaintance and social and professional intercourse among its members.

To publish and distribute such papers as shall add to classified knowledge in chemical engineering and shall increase industrial activity.

ARTICLE III

MEMBERSHIP

SECTION 1. (*Qualifications for Membership.*) Membership shall consist of two grades: Active and Junior.

ACTIVE MEMBERSHIP shall require the following preparation and training:

All candidates must be not less than 30 years of age and must be proficient in chemistry and in some branch of engineering as applied to chemical problems, and must at the time of election be engaged actively in work involving the application of chemical principles to the arts. All candidates for admission to this Institute are expected to have *expert knowledge of at least one branch of applied chemistry* and must fulfill one of the following requirements:

1. Candidates who hold no degree from an approved university or technical school must have had ten years' experience in chemical technology; five being in responsible charge of operations requiring the elaboration of raw materials, the design of machinery involving chemical processes, or the application of chemistry to industry.

2. Candidates who hold the degree of A. B. (Bachelor of Arts) from an approved university or technical school offering a four-year course must have had at least eight years of practical experience as outlined under No. 1.

3. Candidates who hold the degree of Ch. E. (Chemical Engineer), B. S. (Bachelor of Science), in Chemistry or Chemical Engineering, or E. E. (Electrical Engineer), C. E. (Civil Engineer), or M. E. (Mechanical Engineer), or equivalent degrees from an approved university or technical school offering at least a four-year course, must have had at least five years' practical experience as outlined under No. 1.

4. For candidates who in addition hold the degree of Ph. D. (Doctor of Philosophy) or Sc. D. (Doctor of Science) in Chemistry, the number of years required to earn the higher degree may be deducted from the number of years of experience required.

Junior Membership shall require the following preparation and training:

All candidates must be not less than 23 years of age and must be engaged, at the time of election, in some branch of applied chemistry and must fulfill one of the following requirements:

1. Hold the degree of Ch.E. (Chemical Engineer), B.S. (Bachelor of Science) in Chemistry or Chemical Engineering, E.E. (Electrical Engineer), C.E. (Civil Engineer), M.E. (Mechanical Engineer), or equivalent degree from an approved university or technical school offering at least a four years' course.

2. Have had five years' experience in Applied Chemistry.

Junior Members shall have all privileges of the Institute excepting those of voting, holding office, and wearing the emblem or badge of Active Membership. A suitable emblem or badge of Junior Membership as adopted by the Institute may be worn by the Junior Members. When qualified, a Junior Member may apply for Active Membership, but must do so before reaching the age of 35, otherwise his membership shall expire.

Section 2. (*Applications.*) All applications for membership must be made to the Secretary in writing, and shall embody a concise

statement with the dates of the candidate's professional training and experience, and shall be in a form and in such detail as may be prescribed by the Membership Committee. The applicant for Active Membership shall give the names of at least five members to whom he is personally known. The applicant for Junior Membership shall give the names of at least five persons to whom he is personally known, two of whom shall preferably be members of the Institute. Each of these shall be requested by the Secretary to certify to the training, experience, professional attainment, and standing of the applicant. On receiving a favorable report from at least three of these references, the applicant shall be eligible to recommendation by the Membership Committee.

Section 3. (*Election of Members.*) At stated periods the Secretary shall mail to the members a ballot containing a list of all applicants who have been recommended by the Membership Committee. This list shall contain a detailed statement of each applicant's career and the names of the members who have vouched for him. All ballots shall be returned to the Secretary not later than three weeks after the date of issue. The ballots shall be canvassed by the Membership Committee, who shall report to the Council, who shall then declare each applicant elected for whom at least ninety-five per cent. of all ballots cast are in the affirmative. Provided, however, that any member voting in the negative may address a confidential letter to the Council, stating his objections to the candidate with evidence for the charges made. If the Council upon investigation considers such objections valid, they may declare an election void. A rejected candidate may make application again any time after one year. Persons elected to membership shall be notified at once by the Secretary. They must then subscribe to the rules of the Institute.

Section 4. (*Honorary Members.*) As the result of unusual ability and public recognition on the part of the industrial world, a person may, upon nomination of the Council and a vote of the Society at large, be made an Honorary Member, but at no time shall this number exceed five.

Section 5. (*Expulsions.*) For abuse or misuse of the privileges of the Institute or conduct unbecoming a member in the opinion of the Council, a two-thirds vote of the Council may expel any member of the Institute.

Section 6. (*Dues.*) The entrance fee for Active Members shall be $15.00; Junior Members shall pay no entrance fee; Annual dues

for active members $15.00, for Junior Members $10.00. Junior Members, on becoming Active Members, shall pay an entrance fee of $15.00 less $1.00 per year for each year of their membership as Junior Members. Provided, however, that no entrance fee shall be exacted until the membership shall reach 200.

Any member may anticipate his dues for life by paying in advance such a sum as would be demanded by any reputable insurance association to yield an annuity equal to the annual dues from the time of the agreement until death. Upon resignation, or expulsion, all money so provided is to become the property of the Institute. Any person joining the Institute after the middle of the fiscal year is required to pay one-half of the dues only for that year. Any person in arrears for three months shall be notified by the Secretary. For nonpayment at the expiration of one-half year, a member forfeits the right to vote or to receive the notices of the Association until dues are paid in full. Any member one year or more in arrears of dues may, on vote of the Council, be dropped from the Institute. All members are considered as such unless actual resignations are formally presented and accepted with the full payment of dues. On account of extenuating circumstances, dues may be remitted to any member by a two-thirds vote of the Council.

ARTICLE IV.

OFFICERS.

Section 1. The officers of this Society shall be a President, three Vice-Presidents, a Secretary, a Treasurer, an Auditor, and nine Directors. The officers shall be elected at the annual meeting. The President shall serve one year, the Vice-Presidents for three years each, and the Directors for three years each. The Secretary, Treasurer, and Auditor shall be elected for terms of one year each. At the first annual meeting one Vice-President shall be chosen for one year, one for two years, and one for three years. Three Directors shall be chosen for one year, three for two years, and three for three years. Thereafter, officers shall be chosen annually to serve full terms. The President, Ex-Presidents for the two years succeeding the expiration of their term of office as President, Vice-Presidents, Secretary, Treasurer, and Directors shall constitute the Council of the Institute. The President, Vice-Presidents, and Directors cannot be re-elected within the current twelve months from the expiration of term. The duties of office begin immediately

after election and notification. An acceptance of office must be in writing addressed to the Secretary. Vacancies occurring in any office shall be filled by a majority vote of the Council for the unexpired term. The duties of all officers shall be such as usually pertain to their offices or may be delegated to them by the Council or the Institute.

Section 2. (*Election of Officers.*) After the election at which this Constitution is adopted, the election of officers shall be by letter ballot. The Secretary, at least eight (8) weeks prior to each annual meeting, shall send to every member of the Institute a blank nominating ballot upon which the member may make nominations for the officers and Directors to be elected at the coming annual meeting. The nominating ballot is then to be properly signed and transmitted to the Secretary not later than five (5) weeks prior to the annual meeting. It shall then become the duty of the Secretary to prepare and issue an official ballot upon which shall appear the names of all nominations for office or for Directors which shall have appeared upon at least ten (10) nominating ballots. The official ballots shall be mailed not later than three (3) weeks prior to the annual meeting, one to each member, who shall properly signify on it his choice for the various offices and Directors, and transmit it to the Secretary. At the annual meeting the President shall appoint tellers to whom the Secretary shall deliver all the ballots received by him unopened, and who shall count and announce the vote.

ARTICLE V

COUNCIL

The Council shall have supervision and care of all property of the organization, and shall conduct its affairs according to the Constitution and By-Laws. At each annual meeting it shall present a statement of its proceedings during the year. Eight members of the Council called together by notice from the Secretary shall constitute a quorum, provided, however, that three members may be represented by proxy.

ARTICLE VI.

STANDING COMMITTEES.

The Council shall appoint the following committees:

1. FINANCE.
2. COMMITTEE ON MEETINGS.
3. PUBLICATIONS.

4. Membership.
5. Library.
6. House Committee.

FINANCE COMMITTEE.

The Finance Committee shall have charge of the financial affairs of the Institute. This committee must prepare the budget and approve all expenditures. The Chairman of the Committee may be the Auditor of the Institute.

MEMBERSHIP COMMITTEE.

The Membership Committee shall be constituted of fifteen members, ten of whom may vote by proxy at any meeting. To the Membership Committee all applications for membership shall be referred. It is the duty of this committee to see that no person is admitted to the organization who is not qualified.

COMMITTEE ON MEETINGS.

This committee shall have charge of all meetings of the organization and shall fix dates and places of meeting.

COMMITTEE ON PUBLICATIONS.

This committee shall look after the papers presented to the Institute. If considered expedient, any or all of these papers may be published and distributed to members.

LIBRARY COMMITTEE.

This committee shall have charge of all permanent records, books, papers, pamphlets, etc., and shall obtain and place on file a complete record of all patent literature in reference to chemical engineering.

HOUSE COMMITTEE.

This committee shall look after the social affairs of the Institute, fixing the time and place of entertainments.

ARTICLE VII.

MEETINGS.

The annual meeting of the Association shall be held in December, the exact date to be fixed by the Council.

This Institute shall be governed by its Constitution in conformity with the laws of the United States. All questions shall be decided by majority of votes cast. The Institute shall not be held

responsible for opinions expressed in papers. The name or use of the Institute shall not be tolerated for any commercial purpose.

Upon the adoption of this Constitution officers shall be elected immediately to hold office until the election and installation of their successors.

ARTICLE VIII.

AMENDMENTS TO THE CONSTITUTION.

Any member may propose an amendment by addressing the Secretary. At the first regular meeting thereafter the subject shall be discussed, and if worthy, notice to vote on same shall be posted until the next regular meeting, and written copy of the notice shall be sent to each member. The proposed amendment shall then be discussed in open meeting and can be passed by two-thirds vote of all members of the Institute as the result of letter ballot.

BY-LAWS

ORDER OF BUSINESS.

REGULAR MEETING.

Reading of minutes of last stated meeting.
Miscellaneous announcements.
Reading of papers, discussion, and communications.
Adjournment.

ANNUAL MEETING.

Reading of minutes of last stated meeting.
Miscellaneous announcements.
Stated business.
Annual reports.
Election of officers.
Address of retiring President, etc.
Adjournment.

In all questions requiring parliamentary ruling not provided for by the Rules of the Institute, "Robert's Rules of Order" shall be the governing authority.

CODE OF ETHICS

ARTICLE I.

PURPOSE OF THE CODE:

To define the rules of professional conduct and ethics for the members of the Institute.

ARTICLE II.

THE INSTITUTE EXPECTS OF ITS MEMBERS:

1st. That in all their relations, they shall be guided by the highest principles of honor.

2d. The upholding before the public at all times of the dignity of the chemical profession generally and the reputation of the Institute, protecting its members from misrepresentation.

3d. Personal helpfulness and fraternity between its members and toward the profession generally.

4th. The avoidance and discouragement of sensationalism, exaggeration and unwarranted statements. In making the first publication concerning inventions or other chemical advances, they should be made through chemical societies and technical publications.

5th. The refusal to undertake for compensation work which they believe will be unprofitable to clients without first advising said clients as to the improbability of successful results.

6th. The upholding of the principle that unreasonably low charges for professional work tend toward inferior and unreliable

work, especially if such charges are set at a low figure for advertising purposes.

7th. The refusal to lend their names to any questionable enterprise.

8th. Conservatism in all estimates, reports, testimony, etc., especially in connection with the promotion of business enterprises.

9th. That they shall not engage in any occupation which is obviously contrary to law or public welfare.

10th. When a chemical engineer undertakes for others work in connection with which he may make improvements, inventions, plans, designs or other records, he shall preferably enter into a written agreement regarding their ownership. In a case where an agreement is not made or does not cover a point at issue, the following rules shall apply:

a—If a chemical engineer uses information which is not common knowledge or public property, but which he obtains from a client or employer, any results in the form of plans, designs or other records shall not be regarded as his property, but the property of his client or employer.

b—If a chemical engineer uses only his own knowledge or information or data, which by prior publication or otherwise are public property, and obtains no chemical engineering data from a client or employer except performance specifications or routine information, then the results in the form of inventions, plans, designs or other records should be regarded as the property of the engineer and the client or employer should be entitled to their use only in the case for which the engineer was retained.

c—All work and results accomplished by the chemical engineer in the form of inventions, plans, designs or other records, or outside of the field for which a client or employer has retained him, should be regarded as the chemical engineer's property.

d—When a chemical engineer participates in the building of apparatus from designs supplied him by a client, the designs remain the property of the client and should not be duplicated by the engineer nor anyone representing him for others without express permission.

e—Chemical engineering data or information which a chemical engineer obtains from his client or employer or which he

creates as a result of such information must be considered confidential by the engineer; and while he is justified in using such data or information in his own practice as forming part of his professional experience, its publication without express permission is improper.

f—Designs, data, records and notes made by an employee and referring to his employer's work, should be regarded as his employer's property.

g—A client does not acquire any exclusive right to plans or apparatus made or constructed by a consulting chemical engineer except for the specific case for which they were made.

11th. A chemical engineer cannot honorably accept compensation, financial or otherwise, from more than one interested party, without the consent of all parties; and whether consulting, designing, installing or operating, must not accept compensation directly or indirectly from parties dealing with his client or employer.

When called upon to decide on the use of inventions, apparatus, processes, etc., in which he has a financial interest, he should make his status in the matter clearly understood before engagement.

12th. The chemical engineer should endeavor at all times to give credit for work to those who, so far as his knowledge goes, are the real authors of such work.

13th. Undignified, sensational or misleading advertising is not permitted.

14th. Contracts made by chemical engineers should be subject to the Code of Ethics unless otherwise specified.

ARTICLE III.

For the administration of this Code of Ethics, a Committee on Ethics shall be appointed by the president holding office at the time of the adoption of this Code with the approval of the Council, to consist of five members; one appointed for five years, another for four years, another for three years, another for two years, another for one year, and thereafter, the president then holding office shall appoint one member annually to serve for five years and also fill such vacancies as may occur for an unexpired term. All of these members shall be over forty years of age. The Committee shall elect its own chairman. The Committee on Ethics shall investigate all complaints submitted to them bearing upon the professional con-

duct of any member, and after a fair opportunity to be heard has been given to the member involved, shall report its findings to the Council, whose action shall be final.

ARTICLE IV.

Amendments.

Additions to or modifications of this Code may be made according to Article VIII of the Constitution.

OFFICERS AND COMMITTEES FOR 1917

COUNCIL

Elected at the New York Meeting, January 10, 1917

President,

G. W. Thompson ..... Brooklyn, N. Y.

Vice-Presidents,

A. C. Langmuir ..... Brooklyn, N. Y.
John M. Stillman ..... Stanford University, Cal.
Henry Howard ..... Boston, Mass.

Secretary,

John C. Olsen ..... Brooklyn, N. Y.

Treasurer,

F. W. Frerichs ..... St. Louis, Mo.

Auditor,

H. M. Kaufmann ..... New York, N. Y.

Ex-Presidents,

M. C. Whitaker ..... New York, N. Y.
Geo. D. Rosengarten ..... Philadelphia, Pa.

Directors for One Year

Geo. P. Adamson ..... Easton, Pa.
J. B. F. Herreshoff ..... New York, N. Y.
S. P. Sadtler ..... Philadelphia, Pa.

Directors for Two Years

A. D. Little ..... Boston, Mass.
Chas. F. McKenna ..... New York, N. Y.
R. K. Meade ..... Baltimore, Md.

Directors for Three Years

L. H. Baekeland ..... Yonkers, N. Y.
M. H. Ittner ..... Jersey City, N. J.
James R. Withrow ..... Columbus, Ohio

COMMITTEE ON PUBLICATIONS

Olsen, J. C., *Chairman*
Gray, Chas. W.
Hart, Edw.
Bain, J. W.
Ittner, M. H.
Cushman, A. S.
Miner, H. S.

MEMBERSHIP COMMITTEE

Langmuir, A. C., *Chairman*
Adamson, G. P.
Alexander, Jerome
Bassett, Wm. H.
Cushman, A. S.
Dorr, J. V. N.
Wesson, David
Kaufmann, H. M.
Kingsbury, P. C.
Ittner, M. H.
Meade, R. K.
Miner, H. S.
Vorce, L. D.

CHAIRMEN OF LOCAL COMMITTEES ON MEMBERSHIP

Frerichs, F. W. ..... St. Louis, Mo.
Belden, A. W. ..... Pittsburgh, Pa.
Lee, F. H. ..... Cleveland, O.
Miner, H. S. ..... Gloucester City, N. J
Parker, T. J. ..... New York, N. Y.
Howard, Henry ..... Boston, Mass.
Converse, W. A. ..... Chicago, Ill.
Corse, Wm. M. ..... Buffalo, N. Y.

COMMITTEE ON CHEMICAL ENGINEERING EDUCATION

Withrow, Jas. R., *Chairman*
Dow, Herbert H.
Herreshoff, J. B. F.
Little, A. D.
James, J. H.

COMMITTEE ON MEETINGS

Olsen, J. C., *Chairman*
Alexander, Jerome
Bartow, Edward
Baskerville, Chas.
Booth, Wm. M.
Corse, Wm. M.
Frerichs, F. W.
Ittner, M. H.
Lidbury, F. A.
Meade, R. K.
Whitaker, M. C.

FINANCE COMMITTEE

Kaufmann, M. H., *Chairman*
Toch, M.
Zinsser, F. G.

COMMITTEE ON MEDAL

Booth, Wm. M., *Chairman*
Richards, J. W.
Reese, Chas. L.
Sadtler, S. P.

LIBRARY COMMITTEE

Alexander, Jerome, *Chairman*
Olsen, J. C.
Myers, R. E.

COMMITTEE ON PATENTS

Baekeland, L. H., *Chairman*
Toch, Maximilian
Grosvenor, Wm. M.
Lee, F. H.

COMMITTEE ON PUBLIC POLICY

Wagner, T. B., *Chairman*
Baekeland, L. H.
Frerichs, F. W.
McKenna, Chas. F.
Rosengarten, G. D.
Sadtler, S. P.
Whitaker, M. C.

COMMITTEE ON CATALOGS

McKenna, Chas. F., *Chairman*
Whitaker, M. C.
Baekeland, L. H.

LIST OF MEMBERS, JUNE, 1917

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Supt. of the Naugatuck Chemical Co.

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HAANEL, EUGENE, Dept. of Mines, Ottawa, Ont., Can.
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Charge of Operating, Armour Fertilizer Works, Chicago, Ill.

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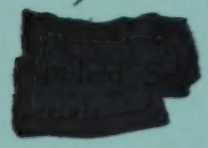

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